

Finnish first-time homebuyer's transfer tax exemption:

Evidence on the effects of transfer taxes

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Tiivistelmä – Referat – Abstract <p>Since 1991, Finland has subsidized homeownership with first-time homebuyer's stamp duty and transfer tax exemptions. Under certain conditions, buyers with ownership shares of at least 0.5 are exempted from paying a tax of 2% on the free-of-debt price for housing company dwellings and 4% on the free-of-debt price for directly owned houses. The previous empirical literature suggests that transfer taxes may lead to large reductions in housing transactions, household mobility, and housing prices. This thesis studies whether there is evidence of similar effects among first-time homebuyers by focusing on the first-time homebuyer's transfer tax exemption.</p> <p>The analysis is based on microdata provided by Statistics Finland and Tax Administration on all permanent residents in Finland and housing company shareholdings. Therefore, this study is limited to housing company dwelling transactions, and it does not cover the effects on directly-owned house transactions. The effect on the first-time home purchase decision is studied using a regression discontinuity design among a subgroup that has not lived in owner-occupied dwellings in adulthood except potentially in the household of their parents. The effect on housing prices is studied using a fixed-effects regression model.</p> <p>The findings show that first-time home purchases drop by roughly 30% at the age threshold of 40 years, where buyers become ineligible for the tax exemption. Similarly, covariate-adjusted estimates show that tax-exempted purchases are on average roughly 1% more expensive than purchases without the tax exemption. If the underlying identification assumptions hold, these estimates can be interpreted as the causal effects of the tax exemption. However, there are potential threats to internal validity. The credibility of the assumptions is studied by conducting graphical and formal tests that typically accompany regression discontinuity designs. There is some evidence consistent with the possibility that the assumptions do not hold, but the evidence is also consistent with alternative explanations related to data limitations. Neither possibility can be ruled out definitively.</p>			
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1 Introduction

Many countries levy taxes on real estate transactions usually by either stamp duty or (real estate) transfer taxation, although it is widely regarded as an inefficient way of collecting revenue among economists (Mirrlees et al., 2011). Also, many OECD countries promote homeownership by a multitude of different housing policies, for example by mortgage guarantees and interest deductions, grants, tax credits, and exemptions as well as by not taxing imputed rental income or capital gains on a primary residence (OECD, 2019a,b). Countries like Canada, Croatia, Finland, Greece, Ireland, Italy, the UK, and the US currently promote, or have in the past promoted, homeownership by tax exemptions that have been specifically targeted to first-time homebuyers.

For example, in Finland, a first-time homebuyer may — under certain conditions — be exempted from having to pay a transfer tax of either 2% of the free-of-debt price for housing company shares or 4% for real estate property. Eligibility for the tax exemption requires that the buyer is between 18 and 39 years old and purchases an ownership share of at least 0.5. The buyer must not have previously owned an ownership share of 0.5 or higher of any apartment or house. The dwelling must be used as a permanent home, and the buyer is required to move in within the following six months after signing the contract. The relative importance of transfer taxes as a source of government revenue varies across countries. In Finland, where transfer taxation amounted to €857 million, or 1.7% of the central government’s tax revenue in 2019 (Statistics Finland, 2021b), the Ministry of Finance (2020) calculates that the annual static cost of first-time homebuyer’s transfer tax exemption is €105 million in years 2019–2021.

When the tax exemption was originally discussed in the parliament, the publicly announced goal was to promote homeownership among young adolescents. Because the tax exemption is costly in terms of lost revenue, it is policy-relevant to figure out whether it has succeeded in reaching its goals. However, there are no previous studies on this particular topic, and the primary goal of this thesis is to contribute to filling this gap. This thesis also contributes to the recent literature on the effects of transfer taxes by using Finnish administrative microdata. The only previous study on this topic from the Finnish context is by Eerola et al. (2019) on how transfer taxes affect household mobility and the nature of the moves. Unlike other studies, this thesis focuses on how transfer taxes impact the decision to become a first-time homebuyer. Additionally, this thesis contributes to the literature by studying how free-of-debt prices of housing company dwellings differ in transactions that are tax-exempted from transactions that are not.

I use regression discontinuity design to study the effects of first-time homebuyer’s tax

exemption at the age threshold where individuals have just become ineligible to claim the tax exemption, at age 40. The analysis is restricted to those individuals who have never lived in owner-occupied dwellings in adulthood except in their parents' households. The analysis is also restricted to housing company share purchases instead of directly owned dwellings for which the tax exemption induces even higher incentives because they are taxed at a higher transfer tax rate. The microdata for this exercise comes from Statistics Finland and Tax Administration. It contains data on all permanent residents in Finland starting from the year 1987 and all housing company shareholdings from years 2005–2016. I find that the likelihood of becoming a first-time homebuyer drops substantially at age 40, but there are some caveats why this may not reflect long-term changes in homeownership rate. There are also data limitations that are thoroughly discussed in chapter 4. In addition, I conduct graphical and formal sensitivity analysis that typically accompanies any study based on regression discontinuity design.

To complement the regression discontinuity design based analysis, I use multivariate regression analysis to study how the free-of-debt prices of housing company dwellings differ between transactions that are tax-exempted and those that are not. I introduce covariates cumulatively into the model and study how the coefficient on tax exemption changes. I find that after adjusting for buyer's covariates, apartment size, year and municipality-fixed effects, tax-exempted housing company dwelling transaction are on average 1% more expensive than transactions without the transfer tax exemption. This would seem to suggest that part of the benefits of the tax exemption accrue to the sellers. However, section 5.4 discusses why the assumptions necessary for this interpretation may not be satisfied.

In the literature review, I focus on two separate strands of literature that are relevant for the tax exemption in terms of justifying homeownership subsidies and understanding what we already know of the effects of transfer taxes.

First, I briefly summarize the literature on the external benefits of homeownership literature, which the argument in favour of subsidizing homeownership in general — not just through this particular tax exemption — is based on. In the economics literature, the case for subsidizing homeownership relies on the positive externalities that homeownership may convey to other individuals in the neighbourhood or the children of homeowners. If such positive externalities exist, there may be too few homeowners because potential buyers do not fully internalize the benefits that accrue to others. Based on this literature, placing an upper bound of age 40 to the tax exemption would be justified if positive externalities below 40 — but not above — are large enough that they outweigh the costs of the policy. Well-known early studies, most notably Green and

White (1997) and DiPasquale and Glaeser (1999) found evidence consistent with homeownership yielding positive externalities. Afterwards, subsequent studies with better data and identification strategies have questioned whether such externalities do indeed exist, and the jury is still out on this issue. However, if such benefits exist, they are not as large as once imagined and the cost-effectiveness of homeownership subsidies should be weighed against other policies that promote the same outcomes. For cost-effectiveness, it is crucial to find out how well the subsidies succeed in promoting homeownership, which this thesis partially studies by focusing on the first-time homebuyer’s transfer tax exemption.

Second, I explore what can be learned from the rather recently emerged empirical literature on the effects of transfer taxes on housing transaction volumes and prices. This section includes a few studies that have analysed previous tax credits that have been specifically targeted to first-time homebuyers. This is supposed to provide a context for the findings of this thesis.

With one exception (Slemrod et al., 2017), all the empirical studies on the effects of transfer tax on transaction volumes suggest that transfer taxes lead to long-term reductions in the number of transactions and household mobility. Transfer tax schedules with notches (US and UK before 2014) may even lead to market unravelling where some transactions with a net-of-tax surplus are not taking place because the transaction price is close to a price notch (Kopczuk and Munroe, 2015). There is also clear evidence of timing effects in anticipation of announced tax rate changes (Besley et al., 2014; Best and Kleven, 2018; Slemrod et al., 2017). These short-term timing effects justify why temporary transfer tax cuts — e.g. the UK in the aftermath of global financial crisis — may be effectively used as partial stimulation measures at downturns. Best and Kleven (2018) provide evidence that a temporary transfer tax (Stamp Duty Land Tax) cut in the UK also spurred additional housing-related consumption at least for the first year after the transaction. In fact, UK has implemented a similar temporary transfer tax cut as a response to COVID-19 that lasts from July 8, 2020 to March 31, 2021. Similarly, studies typically suggest that although in most cases the statutory incidence is on the buyer, at least half (Besley et al., 2014; Slemrod et al., 2017) of the economic incidence falls on the seller — but it may even exceed 100% (Dolls et al., 2019; Kopczuk and Munroe, 2015; Petkova and Weichenrieder, 2017). Some studies have suggested that the effects on prices may be more pronounced for apartments than for single-family houses because apartments tend to be traded more frequently as they are more often purchased for investment purposes (Dolls et al., 2019; Petkova and Weichenrieder, 2017). These studies also reveal that in housing markets, households change their behaviour relatively

fast in response to changes in tax legislation (e.g. Besley et al., 2014; Best and Kleven, 2018). However, the salience of the legislative reforms is important (Slemrod et al., 2017). These studies, among others, will be discussed in more detail in chapter 3.

It can be argued how well first-time homebuyer's tax exemption targets those households that are most likely to require additional financial support to become homeowners. In Finland, the policy was promoted based on the financial grounds of supporting young households to begin independent lives in owner-occupied dwellings. However, the eligibility in Finland has never depended on household wealth or income, whether the dwelling is financed by equity or by debt, nor has there been limits to the purchase price. Therefore, those who purchase more expensive dwellings benefit more from this tax exemption than those who purchase less expensive dwellings. This can be contrasted to the UK and the US where similar tax exemptions have been targeted to lower- to moderate-income households. For example, in the UK after COVID-19, first-time homebuyers are exempted from stamp duty on the first £300,000 of the purchase price. In the US after the global financial crisis, a First-Time Homebuyer Tax Credit was phased out after a certain income threshold. Currently, a low-income household that manages to accrue enough savings for down payment only after turning 40 years old does not benefit from this tax exemption, unlike high-income household that decides to become homeowner before age 40. There is also the additional question of whether transfer taxation is a good way of collecting revenue in the first place, or whether it should be replaced revenue-neutrally by other taxes, as many have suggested (e.g. Mirrlees et al., 2011). These questions will not be pursued further here.

The rest of the thesis proceeds as follows. In the next chapter, we briefly discuss the origin of the first-time homebuyer's transfer tax exemption. This is followed by the literature review chapter that is divided into two sections. The first section reviews shortly the literature on the external benefits of homeownership, and the second reviews the empirical evidence on the effects of transfer taxes as well as evidence on the effects of first-time homebuyer tax credits. Chapter 4 presents the data and the regression discontinuity design. We also discuss data limitations and try to justify why certain decisions in analysing the data were chosen. Finally, chapter 5 presents the results with sensitivity analysis. Finally, chapter 6 concludes with a summary of the findings and discusses how they should be interpreted.

2 Origin of the first-time homebuyer's tax exemption

In the literature, the main argument for subsidizing homeownership relies on the potential externalities of homeownership. However, these arguments — discussed in section 3.1 — were, to the best of my knowledge, never explicitly put forward in Finland. Around the time when this tax exemption was implemented, the main argument in public discourse was to give financial support to young people who were starting their independent lives. This chapter briefly discusses how and why the tax exemption currently exists in the transfer tax legislation.

Transfer taxation was introduced into the Finnish tax legislation on January 1, 1997. It replaced stamp duty tax legislation which was originally passed in 1943 and was still in its original form in many ways (Government Proposal, 1996). First-time homebuyer's tax exemption was part of the transfer tax legislation from the very beginning. The tax exemption was originally introduced by a law amending the Stamp Duty Act (1167/1990) that took its effect on January 1, 1991. The legislation also applied to those dwellings that were bought after November 1990, allowing the market to function after the legislation was publicly announced. The current eligibility criteria are the same as they were originally when the law was passed with a small exception. In 1990–1995 the requirement of not having previously owned an ownership share of 0.5 or higher was only applied to the transaction year and the five previous years. For example, a person who had owned and sold a single-family detached house in 1987 and lived as a renter afterwards was eligible for the tax exemption in 1994, given that the other conditions were satisfied. Starting from 1996 an eligible buyer was not allowed to have owned an ownership share of 0.5 or above after the year 1989.

The tax exemption was discussed for quite some time before the legislation passed. According to the minutes of the Parliament (1990, pp. 3242–3248), a member of the National Coalition Party claimed that the Information Service of the Parliament had confirmed that a member of his party back in 1984 was the first in ten years to propose the tax exemption. Based on the same minutes of the parliament, it seems that the tax exemption shared unanimous support across the political spectrum, and the discussions in the parliament were largely about which party should be credited for having been the first one to propose it. Otherwise, the discussions were about whether the tax exemption should be extended to a wider population as well, and not just to first-time homebuyers. Before passing, when the legislation was discussed in the Finance Committee, two Left-Alliance members proposed extending the tax exemption to all dwellings that were used as permanent homes, but this proposal was voted down (Finance Committee, 1990).

Section 2.5 in the Additional Protocol to the Government's Programme in 1987 states that the goal in stamp duty tax reform is to improve the financial situation of first-time homebuyers. House price appreciation had been considerable and stamp duty was seen as a major expenditure in buying a home (Government Proposal, 1990). The Finnish housing prices had increased sharply at the end of the 1980s peaking in the final quarter of 1989. Over the next three years, housing prices came steadily down nationwide, on average by over 50%. Combined with high interest rates and shortage of rentable apartments, young adults specifically were considered to be in a difficult situation, and this explains why the tax exemption is restricted to those below age 40. The lower age limit of 18 years was set in order to target the exemption to those who have had the opportunity to earn the required funds by themselves, and to prevent parents from using gifts and advanced inheritance to purchase housing with the help of their children (Government Proposal, 1990).

A Memorandum by the Working Group on Transfer Taxation (1990) suggested against implementing the tax exemption because it required drawing demarcation lines in many cases. For example, it was unclear who should be considered as a first-time homebuyer, how to define a permanent residence, and whether residing there can be monitored. The working group concluded that there were more efficient ways to target financial support to first-time homebuyers through the tax code.

3 Literature review

3.1 External benefits of homeownership

The primary argument in favour of subsidizing homeownership relies on positive externalities that it may convey to neighbours and the children of homeowners. If such benefits exist, and homebuyers do not internalize these to the full extent, there will be too few homeowners than what is socially optimal. This justifies subsidizing homeownership if the external benefits outweigh the costs. It has been argued that homeownership may increase neighbourhood-specific social capital investments and political participation (e.g. DiPasquale and Glaeser, 1999; Hilber, 2010; Hoff and Sen, 2005; Holian, 2011), housing maintenance (e.g. Galster, 1983; Iwata and Yamaga, 2008; Mayer, 1981; Shilling et al., 1991), and benefit the children of homeowners (e.g. Aaronson, 2000; Green and White, 1997; Harkness and Newman, 2003; Haurin et al., 2002). Among all the potential benefits that homeownership may have, the effects on children have been considered to be “potentially the most far reaching” (Dietz and Haurin, 2003, p. 430) and perhaps to constitute “the best argument for subsidizing homeownership” (Glaeser and Shapiro, 2003, p. 71).

The main explanation why homeownership may affect these outcomes relates to incentives. Homeowners and renters both benefit directly from improved neighbourhood quality, but homeowners benefit also indirectly because the better quality gets partially capitalized into housing prices (DiPasquale and Glaeser, 1999), especially in areas with inelastic supply of new housing (Hilber, 2010). Potential buyers and tenants value neighbourhood quality and are willing to pay more for better quality neighbourhoods. For renters, improved neighbourhood quality leads to higher rental rates. Therefore, homeowners are better incentivized to participate in the long-term development of their communities. Pride and social status have also been suggested to play a role when it comes to maintenance decisions (Galster, 1983; Rohe and Stewart, 1996), and homeowners may also be better aware of improvements that potential homebuyers and tenants value in the area (Galster, 1983; Mayer, 1981). Better maintenance may in some cases improve the health of children which may affect their outcomes in later life (Haurin et al., 2002). Green and White (1997) suggest that homeownership teaches management skills that may be transferrable onto creating a better home environment for children. They also hypothesize that homeowners may become better at monitoring their children, partly because of better incentives to make sure they do not misbehave and reduce the neighbourhood quality and partly because homeownership may reduce household mobility.

It is acknowledged that homeowners differ from renters in many ways (Rossi and Weber, 1996) that are consistent with these hypotheses. However, the most difficult thing in the empirical literature has been to overcome the endogeneity of homeownership — whether tenure status is driving the differences, or whether those who become homeowners are just inherently different from those who rent. Prominent early studies that attempted to solve the endogeneity (DiPasquale and Glaeser, 1999; Green and White, 1997) claimed to find evidence that was consistent with homeownership systematically leading to better outcomes. It is, however, difficult to find instrumental variables that satisfy exogeneity, and in many cases instrumenting homeownership leads to inflated point estimates and standard errors. Often the authors themselves prefer the uninstrumented results (DiPasquale and Glaeser, 1999; Hilber, 2010). More recent studies (Barker and Miller, 2009; Engelhardt et al., 2010; Holupka and Newman, 2012) with better data and methods have been unable to find evidence that homeownership has the presumed effects that may yield external benefits.

Some studies (Coulson et al., 2002, 2003; Coulson and Li, 2013; Glaeser and Shapiro, 2003; Kortelainen and Saarimaa, 2015; Rohe and Stewart, 1996) have taken an alternative, indirect approach to study whether homeownership yields benefits to the neighbourhood by comparing whether homeownership rate is associated with higher housing prices. Insofar as these benefits exist and are valued by potential buyers, *ceteris paribus*, neighbourhoods with higher homeownership rates should have higher housing prices. The benefits of this approach are that the researcher does not have to specify and observe all the externality-yielding mechanisms, and it quantifies how these benefits are valued by potential homebuyers. However, in this approach it is necessary to be able to adjust for other neighbourhood-level covariates that affect housing prices, and to deal with simultaneity — house prices also affect homeownership rates.

The most prominent US studies (Coulson et al., 2002, 2003; Coulson and Li, 2013) find evidence suggesting that homeownership rates in small, roughly 10 unit, single-family detached house clusters are indeed associated with higher house prices. Although these clusters are considered bad approximations to neighbourhoods due to their limited size (Rohe and Stewart, 1996, footnote 4), external benefits are likely to accrue to the closest neighbours. On the other hand, Kortelainen and Saarimaa (2015) who using a similar approach studied multi-storey apartments in Helsinki were unable to find evidence that is consistent with external benefits.

To conclude, based on the current state of this literature, there is no strong evidence to support the claim that homeownership has the sort of causal effects that arguments in favour of subsidizing homeownership rely upon. More research with credible identifi-

cation strategies and good data would be beneficial. We will not get further into topic in this thesis. It is briefly presented here because the Finnish first-time homebuyer’s transfer tax exemption is a special case of homeownership subsidy and, therefore, this literature is relevant for justifying its existence.

3.2 Evidence on transfer taxes and first-time homebuyer tax credits

Property transfer taxes, or stamp duties, are widespread among the EU and the OECD countries, and in many cases, they constitute a substantial fraction of the total transaction costs (Andrews et al., 2011; European Commission, 2014). Transfer taxes resemble capital gains taxes in the sense that tax liability is triggered in both cases by a transaction and can therefore be avoided by choosing not to transact. However, with property transfer taxes, the tax falls on the full purchase price and not just on the appreciation. Because tax liability is triggered by a transaction, by nature, transfer tax falls more heavily on those households who choose to transact more often, whatever the reason. Therefore, transfer tax — as well as any other increase in transaction costs like registration, notarial, and real estate agency fees (Andrews et al., 2011) — incentivizes frequent movers to the rental markets. It is well understood that transfer taxes reduce the potential for mutually beneficial trades among buyers and sellers because it increases the required net-of-tax surplus for the transaction to occur. Therefore, it may reduce housing transaction volumes creating a so-called “lock-in” effect, where households who in absence of the tax would have up- or downsized their dwelling or moved due to labour market-related reasons are discouraged by the tax to do so (Eerola et al., 2019; Hilber and Lyytikäinen, 2017). In other words, households may live in dwellings that are sub-optimal in size or location to avoid the tax.

Because of the reasons outlined above, a common recommendation in papers on housing transfer taxes is to replace the distortive transfer tax with recurrent taxes on housing investments and consumption that cannot be avoided by choosing not to transact. The widely-cited work when it comes to the design of modern tax legislation — the Mirrlees Review — expresses this opinion clearly: “In no case do we find the arguments for transactions tax compelling” (Mirrlees et al., 2011, p. 151) and “There is no sound case for maintaining stamp duty and we believe that it should be abolished” (Mirrlees et al., 2011, p. 404). Estimates of the deadweight losses and marginal costs of funds vary quite a bit depending on the assumptions (see Besley et al., 2014; Buettner, 2017; Dachis et al., 2012; Eerola et al., 2019; Hilber and Lyytikäinen, 2017; Slemrod et al., 2017), but are generally not negligible and in some cases rather sizable.

This chapter reviews the empirical literature on the effects of transfer taxation. Most studies are focused to uncover how transfer taxes affect transaction volumes, household mobility, and prices — how is the economic incidence divided between buyers and sellers. These studies on the effects of transfer taxes will be discussed in more detail shortly, but first, we discuss the small literature there exists on the effects of tax credits that are specifically targeted to first-time homebuyers.

3.2.1 First-time homebuyer tax credits

In general, it is difficult to find studies on the effectiveness of subsidy policies that are specifically targeted to first-time homebuyers. This is surprising considering the number of OECD countries that have a multitude of different first-time homebuyer subsidy policies in place (OECD, 2019a,b). Hembre (2018) studies the effectiveness of a refundable first-time homebuyer tax credit policy that was implemented in the aftermath of the housing market crash in 2007–2008. Similar to the UK, which temporarily increased the first tax threshold for Stamp Duty Land Tax, the US government responded by passing three acts (HERA, ARRA, and WHBAA) over the 2008–2010 period. These acts included First-time Homeowner Tax Credit (FHTC) that was in effect from April 2008 to June 2010 to boost housing and consumption demand and to prevent foreclosures and bank losses (Dynan et al., 2013). The first two iterations were targeted only to first-time homebuyers whereas repeat buyers were also eligible in the third iteration. In the first iteration, FHTC was effectively a zero-interest loan because it had to be paid back. During ARRA and WHBAA this was no longer the case, and the maximum credit was increased up to \$8,000 (Dynan et al., 2013; Hembre, 2018).

It is clear that the share of first-time homebuyers increased during the last two policy phases (Dynan et al., 2013; Hembre, 2018), but quantifying the precise effect of the tax credit at such an exceptional time is difficult. FHTC was part of larger stimulus packages, it coincided with the Fed’s QE policy and some states created additional subsidies for first-time homebuyers. Dynan et al. (2013) try to study the overall effects of FHTC policies on housing markets, but their time-series forecasting models and diff-in-diff strategies yield mixed results and are not robust. Hembre (2018) uses a different diff-in-diff strategy by using the repeat buyers as a control group for true first-time homebuyers during the first two policy phases. He estimates that the first two iterations increased first-time home purchases by 16%, and during the second iteration, without mandated payback, the effect exceeded 20% (Hembre, 2018). A potential caveat, in addition to the existence of other simultaneous policies, is whether repeat buyers provide a good

control group i.e. whether the trends would have developed similarly without the tax credit. The observed increase may also have been due to short-term retiming decisions, even though Hembre (2018) tries to address this issue and estimates that only 15% were due to retiming decisions that took place within a one-year horizon.

Another first-time homebuyer tax credit that has been studied was implemented at the state level in Washington D.C. and it was targeted to low- to middle-income households like the FHTC. Tong (2005) studies this maximum benefit of \$5,000 over the 1997–2001 period. Although Tong (2005) cites the program as effective in promoting homeownership and documents how first-time homebuyer share in all home purchases in D.C. exceeded city centre average by 16.1 percentage points, he does not provide an estimate of the share that is contributable to the tax credit. Instead, Tong estimates the effects on price appreciation in the diff-in-diff framework by studying how the difference in price appreciation between D.C. and five of its adjacent jurisdictions changed when the policy was implemented. The estimates suggest that the policy increased annual amenity-adjusted price appreciation by 4.9 percentage points from 10.7% to 15.6% and the effect was more pronounced in townhouses and condominiums relative to single-family detached houses. Similar to the FHTC, there were other potentially important changes happening simultaneously in D.C. that may have affected the estimate. It is also plausible that some purchases were shifted forward in time, and like in other border diff-in-diff designs, the tax credit may have affected housing markets in the surrounding jurisdictions by moving some transactions from across the border to D.C.

3.2.2 Transfer taxes

Next, we will review the empirical studies on the effects of transfer taxation. These studies are all from the past decade, and they focus on Australia, Canada, Finland, Germany, the UK and the US. Although some earlier studies do exist, the bulk of the papers that provide empirical evidence on the effects happens to be conducted very recently.

3.2.2.1 UK

Several papers (Besley et al., 2014; Best and Kleven, 2018; Hilber and Lyytikäinen, 2017) have studied the effects of the UK Stamp Duty Land Tax (SDLT) scheme which was in place until December 3, 2014 (Hilber and Lyytikäinen, 2017). Before this, housing transactions within certain bands were taxed at different rates that applied to the full transaction price. Therefore, at certain thresholds — or notches — a marginal increase

in transaction price triggered a significant jump in tax liability. These price notches combined with policies that moved the locations of these notches around have provided one setting where the effects of property transfer taxes have been studied.

As a partial response to the global financial crisis, on September 3, 2008, the UK announced an unanticipated Stamp Duty Holiday which came into effect immediately and lasted until the end of the year 2009. This policy temporarily moved the threshold where the SDLT rate jumped from 0% to 1% from £125,000 to £175,000. Besley et al. (2014) study the effects of this policy using data from the Financial Services Authority that contains property evaluations in case of mortgage defaults, conducted by independent contractors. It is argued that these valuations are unlikely to be affected by the tax holiday as the potential default is likely to happen only after the temporary policy is over. Using difference-in-differences identification strategy, Besley et al. (2014) estimate that the removal of a 1% SDLT rate decreased housing prices on average by roughly 0.6% in the affected range of £125,000–£174,999. They also estimate that transactions increased by 8%, but the majority of this is driven by the large number of purchases that took place just before the anticipated end of the policy and only a small number of purchases right afterwards. Therefore, the observed increase in transactions may largely be due to re-timing decisions instead of permanent increases in the number of transactions.

On the other hand, Best and Kleven (2018) use administrative data on over 10 million stamp duty returns to study the effects of the same UK Stamp Duty Holiday, with different ways of accounting for the bunching and gap around the notches than Besley et al. (2014). Their estimates suggest that the short-term increases in the number of transactions in the affected range were even larger: a 20% increase in the short-term, of which only around 40% was due to retiming decisions. Best and Kleven (2018) also study price effects of SDLT using price notches at £250,000 and £500,000 where tax liability increases by £5,000 at both notches. They also provide clear evidence of bunching just below the notches and find missing transactions up to £25,000 above the price notches.

By affecting housing transactions, transfer taxes are likely to affect household mobility, but housing transaction does not necessarily mean that a household moves from one unit to another. Hilber and Lyytikäinen (2017) exploit the price notch at £250,000 where the tax rate on the full transaction price increases from 1% to 3% to study how it may have heterogeneous effects on household mobility depending on the type of the move. Using British Household Panel Survey data from the years 1996 to 2006, they estimate that a 2 percentage point increase in transfer tax rate decreased household mobility around the notch by 37% among households that had not moved within the past two years. Hilber and Lyytikäinen (2017) also conduct the analysis stratifying by the

nature of the move, revealing that the decrease in overall mobility is driven by reductions in short (vs. long) and housing and area (vs. employment and major life-event) related moves. Therefore, it seems that at least around the £250,000 notch, the negative effects were confined to housing markets instead of labour markets.

3.2.2.2 USA

Similar to the UK, price notches and associated policy changes over time have also been used in the United States to study the effects of transfer taxes. Kopczuk and Munroe (2015) use administrative data on the states of New York and New Jersey to study the effects of “mansion tax” — a tax of 1% that is levied on the full purchase price of a real-estate property that sells at \$1,000,000 or above. According to their results, the local incidence of the tax falls heavily on the seller, exceeding 100% even after accounting for the fact that sellers may adjust housing quality in response to the tax. In addition, they find evidence suggesting that the large effect is not caused by tax evasion. Furthermore, Kopczuk and Munroe (2015) find that the mass that bunches right below the notch is not sufficient to explain the size of the gap just above the notch. This is interpreted as evidence of local market unravelling, which differs from the usual extensive margin response where the presence of the tax leads to negative net-of-tax surpluses. On the contrary, Kopczuk and Munroe (2015) use market unravelling to refer to the situation where transactions that yield positive surplus and take place in the presence of a proportional tax are not conducted only because of the notched tax scheme. In New York City, the 1% “mansion tax” is estimated to have led to the unravelling of 0.7% of all transactions.

Slemrod et al. (2017) study Washington D.C., where the statutory incidence of the real estate transfer tax is split 50–50 between the buyer and the seller, unlike in the other contexts discussed in the papers of this section. In 2003 transfer tax rate on the full sales price was increased from 2.2% to 3% for units valued at \$250,000 or above for 21 months, and in 2006 transfer tax rate was again increased to 2.9% on transactions at \$400,000 or above. Contrary to Kopczuk and Munroe (2015), Slemrod et al. (2017) do not find statistically significant differences in the amount of bunching below the notches and the gaps above.¹ Perhaps somewhat surprisingly they are also unable to find evidence of extensive margin responses: their difference-in-differences estimates suggest that 2003 and 2006 reforms did not affect transaction volumes away from the notch. Interestingly, in situations where both parties remit part of the tax, there exists a strictly dominated

¹On the other hand, the point estimate for bunching was higher in magnitude. Best and Kleven (2018) also found this, although neither was their difference of the bunching and gap statistically significant.

price region, where both parties benefit if they agree to reduce the purchase price below the notch. Salience through media coverage seems to have been important for this kind of optimization to take place. Slemrod et al. (2017) find that for the 2003 reform, which received little media coverage, there was only a 3% decline in sales in the strictly dominated region, whereas for 2006 reform, with more media coverage, the decline in sales in the dominated region was 68%.

3.2.2.3 Germany

In 2006 Germany implemented a policy change which provides another opportunity to study the effects of transfer taxes. Unlike in the UK and the US, there are no notches to exploit in the German real estate transfer tax (RETT) schedule, where only a single tax rate applies to all residential property transactions. Until the year 2006 the RETT rate was federally set to 3.5% but starting from 2006 states were allowed to set their tax rates but not the tax base (Petkova and Weichenrieder, 2017). By 2013 all but two states had implemented a tax hike and no state had even once decreased the rate (Fritzsche and Vandrei, 2019). This within and between variation over states and time has been exploited by multiple authors using panel regression methods.

Petkova and Weichenrieder (2017) use annual indices of property transactions and average purchase prices at the state level from 2003 to 2014 and are the first to carry out their estimations separately for single-family houses and apartments. According to their estimates, a 1% increase in RETT decreases transactions by 0.23% for single-family houses but they cannot find evidence to reject the hypothesis that apartment transactions are unaffected. However, for prices the results are the opposite: they only find evidence that apartment prices react to transfer taxes and that sellers carry more than 100% of the incidence. The annual indices used by Petkova and Weichenrieder (2017) have been criticised for being inconsistent over states and over time and not being based on complete microdata (Fritzsche and Vandrei, 2019, footnote 6). Buettner (2017) also uses annual state-level data to estimate how RETT changes affected transfer tax revenues in the years 2002–2015. He finds point estimates for the elasticity of tax revenue that range between 0.49 and 0.68 across different specifications, and an average elasticity of 0.6 is Buettner’s (2017) preferred estimate. These estimates fall generally to the range also found in Petkova and Weichenrieder (2017) who find comparative point estimates ranging between 0.58 and 0.72. The deadweight loss of collecting an additional euro in tax revenue through RETT is estimated by Buettner (2017) to be 0.68 cents.

Fritzsche and Vandrei (2019) limit their focus to six German states, for which there

are monthly state-level data from the years 2005–2014, based on the copies of transaction contracts. The focus is exclusively on how single-family house transaction volumes are affected. Based on their estimates, a 1 percentage point increase in RETT rate decreases transactions by 7% due to the lock-in effect — the extensive margin response. In addition, Fritzsche and Vandrei (2019) find that of the overall effect roughly 40% are due to short-term retiming decisions, which is the same magnitude that Best and Kleven (2018) found in their analysis of the UK Stamp Duty Holiday.

Finally, Dolls et al. (2019) use listings data from years 2005–2018 of roughly 18 million properties that were offered for sale. Based on their event study estimates, a 1 percentage point increase in RETT rate decreases the prices of apartments by 3–4%, apartment buildings by 2–4%, and single-family houses by 1.5–2% one year after the reform. The finding that the effects on apartments are more pronounced than on single-family houses corresponds to what Petkova and Weichenrieder (2017) also find. This can be rationalized if the expected holding period is shorter for apartments than for single-family houses as formally demonstrated in both Petkova and Weichenrieder (2017) and Dolls et al. (2019), which could be the case as apartments are more often than single-family houses purchased for investment purposes (Dolls et al., 2019). This also offers one explanation for why prices may decrease over and above the tax increase. Alternatively, Best and Kleven (2018) show that liquidity constraints on downpayments can produce the same results.

3.2.2.4 Australia, Canada, and Finland

Although most of the studies focus on the UK, the US, and Germany, there are some studies from other countries. In early 2008 the City of Toronto implemented an additional 1.1% Land Transfer Tax (LTT) to collect more revenue to finance the budget. Dachis et al. (2012) study this reform by comparing the changes in prices and transaction volumes to changes that took place in bordering municipalities in the Greater Toronto Area. According to the preferred difference-in-differences estimates by Dachis et al. (2012), the imposition of LTT reduced single-family house transactions by 14% and lowered prices by roughly 0.88%². Because the authors conduct their estimations within narrow bandwidths around the border, a potential worry is that the tax in Toronto may not only have affected housing transactions within its borders. Instead, it may also have pushed transactions from within its borders right across the border to the neighbouring municipalities. Whereas Dachis et al. (2012) focused on a narrow bandwidth around the

²The point estimates for price effects range from -0.81 to -1.48 with relatively large standard errors from 0.42 to 0.79 depending on the specification (Dachis et al., 2012, Table 5).

border, the identification strategy used in Dolls et al. (2019) was the opposite as they excluded from their estimations the postal codes that were within 10 kilometres from the borders.

Unlike Germany with their flat transfer tax rate and the UK before the 2014 reform where the increasing rate applied to the full purchase price, Australia has a progressive land stamp duty schedule, with marginal rates levied only on the parts that exceed the corresponding thresholds. This context has been studied by Davidoff and Leigh (2013) who focus on house sales and use data aggregated to the postcode level from years 1993–2005. Because of the progressive schedule, stamp duty rate and hence stamp duty paid are functions of the purchase price, which creates endogeneity that Davidoff and Leigh (2013) account for by using an IV strategy. In specific, Davidoff and Leigh (2013) instrument the taxes paid in a postcode in any given year by the taxes that would have occurred if the postcode’s average house price at a base year had appreciated according to the national price index. Their estimates suggest that a 1 percentage point increase in stamp duty tax rate reduces house transactions by 8% in the short-run and lowers prices by 6%.

Similar to Hilber and Lyytikäinen (2017), a recent study from Finland by Eerola et al. (2019) assesses the effects of transfer taxes on household mobility and on the nature of the moves. Eerola et al. (2019) study the 2013 transfer tax reform which increased the tax rate from 1.6% to 2.0% for housing company dwellings while keeping the rate constant at 4% for directly-owned single-family detached houses. If this setting is analysed as a typical difference-in-differences setup, the reform is estimated to have decreased household mobility by 5.6% for those residing in housing company dwellings. However, Eerola et al. (2019) explicitly account for the fact that the tax reform may also affect single-family detached house markets because some owner-occupiers move from single-family detached houses to apartments, but the tax increase reduces the potential for such mutually beneficial trading opportunities. Their solution is to calibrate a theoretical housing market model to reproduce their diff-in-diff estimates, and to use this model to provide an estimate of the true reduction in household mobility in response to the tax increase. The calibrated model suggests that the reduction in household mobility is 7% instead of 5.6% suggested by the diff-in-diff estimate. In addition, Eerola et al. (2019) find that cross-region moves were largely unaffected, but there were differences in whether the moves took place across or within municipalities. This is consistent with the notion that labour market-related moves may have been less affected than housing consumption-related moves as in the UK (Hilber and Lyytikäinen, 2017), but some ambiguity remains. Similarly, there is no evidence that moves related to downsizing were

affected whereas moves related to upsizing were reduced by the tax.

To summarize, the empirical evidence presented in this section, with one exception (Slemrod et al., 2017) suggests that transfer taxes lead to long-term reductions in housing transactions and decrease household mobility. Anticipated changes in transfer tax schedules are also shown to have large short-run timing effects (Besley et al., 2014; Best and Kleven, 2018; Slemrod et al., 2017). The studies also show that transfer taxes reduce housing prices and the magnitude may depend on the building type (Dolls et al., 2019; Petkova and Weichenrieder, 2017). Even though the statutory incidence is on the buyer, at least half of the economic incidence seems to fall on the seller, but the seller may even bear more than 100% of the tax (Dachis et al., 2012; Davidoff and Leigh, 2013; Dolls et al., 2019; Kopczuk and Munroe, 2015). Comparing studies with different data and methodologies across countries is not straightforward, but a fair summary could be that a one percentage point increase in transfer taxes is generally estimated to reduce housing transactions roughly between 7% and 15%. For the Finnish context, the most relevant study (Eerola et al., 2019) suggests that a 0.5 percentage point increase in transfer taxes reduces household mobility by roughly 7%.

It is more difficult to draw predictions of how first-time homebuyers in Finland are affected by transfer taxes — how much they and repeat buyers behave alike. The situation in the housing markets after the global financial crisis when the US implemented its First-Time Homebuyer Tax Credit program (Dyner et al., 2013; Hembre, 2018) was exceptional and it collided with other simultaneous policy measures. At face value, Hembre (2018) estimates suggest that the tax credit policy that was capped at \$8,000, corresponding roughly to 3% of the purchase price, may have increased first-time homebuying roughly by 17% or more in the long-term. A tax credit of roughly 3% of the mean purchase price is in the middle of the Finnish transfer tax rates for housing company dwellings (2%) and directly owned houses (4%), so the effects could potentially be sizable in Finland as well. However, basing predictions of the effects in Finland on the existing literature on first-time homebuyer tax credits rests on shaky grounds. Similarly, it is unclear whether we expect to observe higher prices on first-time home purchases. Some have suggested that “First-time owner subsidies that are not tied to specific houses should not be expected to be capitalized” (Hendershott and White, 2000, p. 270).

The next chapter presents the methods and data that I use in my attempt to gain more information regarding the effects on first-time homebuyers in Finland.

4 Methods and data

In Finland, homebuyers are exempted from having to pay transfer tax when they purchase their first home if certain eligibility criteria are met. According to the Transfer Tax Act (29.11.1996/931)³, the buyer must be at least 18 and at most 40 years old, and the buyer must own 0.5 or above of the ownership share. Furthermore, the buyer will have to buy the dwelling for permanent residency, and the buyer cannot have previously owned an ownership share of 0.5 or above of an apartment or house. If these criteria are not met, the buyer will have to pay a transfer tax rate of 2% of the free-of-debt sales price for housing company shares and 4% of the sales price for real-estate and buildings. Prior to March 1, 2013, housing company share transactions were taxed at a rate of 1.6% and the tax base was only the transaction price. After the reform, the tax base consisted of the free-of-debt price that includes the share of the housing company's loans that the buyer becomes responsible for, and the tax rate was increased to 2% (Eerola et al., 2019).

The research question that motivates the empirical exercise in this thesis is how first-time homebuyer's transfer tax exemption affects the decision to become a first-time homebuyer. Ideally, we would like to compare the behaviour of identical individuals in two separate worlds that are otherwise similar, except that one has a transfer tax on housing transactions while the other does not. However, this is clearly not feasible.

The approach in this thesis is to study the tax exemption as a sharp regression discontinuity (RD) design. RD provides a credible research design to identifying causal effects under assumptions that are easily understood. Among published studies, those based on RD design have been less prone to p-hacking than those based on IV and DiD that are also common in causal inference (Brodeur et al., 2020). RD is typically accompanied by a multitude of sensitivity tests that are used to study the credibility of its underlying assumptions, and graphical evidence that makes the RD design easy to communicate and transparent (Lee and Lemieux, 2010). Due to the nature of RD, the effects on first-time homeownership are studied at age 40, where the individuals will no longer be eligible to transfer tax exemption because they no longer satisfy the age criterion. Instead of studying the effect in the general population, for reasons discussed in the data section, we will focus on a specific population, which may be more policy-relevant considering the original motivation for the transfer tax exemption legislation. Based on the literature review, we may expect to see a noticeable discontinuity in the likelihood of becoming a homeowner if we compare eligible individuals to otherwise

³Available in Finnish at <https://finlex.fi/fi/laki/ajantasa/1996/19960931>.

similar ineligible individuals.

We proceed by first introducing the RD design with a special emphasis on the sharp, as opposed to the fuzzy, version of the design. Next, we go through how the estimation is conducted and some of the difficulties that are caused by the discretely measured age variable. Finally, we discuss the data that is used in the estimation as well as its limitations.

4.1 Methods

RD was first introduced by Thistlethwaite and Campbell (1960) in a paper, that studied the effects of public student recognition. In that setting, whether a student received a Certificate of Merit or not, was primarily dependent on test scores in a Scholarship Qualification Test. The key idea of Thistlethwaite and Campbell 1960 was that students just below the test score threshold for the certificate could be used to extrapolate for those students who barely crossed the qualification threshold and were awarded the certificate. This extrapolation would then serve as a counterfactual to what would have happened to students who scored just above the threshold, had they not received the certificate. Thistlethwaite and Campbell 1960 considered RD design to be a useful alternative in settings where conducting randomized experiments is not feasible or considered ethical.

Although RD design made a brief appearance in economics literature already back in the 1970s, it took until the mid-1990s before it became more widely recognized among applied researchers (Cook, 2008)⁴. In the past 20 years, RD design has gained more popularity as part of the “credibility revolution” in empirical economics (Angrist and Pischke, 2010) and it has been increasingly used in the estimation of treatment effects in many different fields. Although RD design has also been recently used in education studies, many government policies have also proven to provide settings where RD design-based evaluation can be applied. For example, in housing economics, Ferreira (2010) used RD design to study how California’s Proposition 13 property tax legislation affected household mobility. Even though government policies may not have initially been designed with program evaluation considerations in mind, many government policies have strict rules, e.g. age and income limits or geographic boundaries, which determine eligibility to program participation. It is, therefore, no wonder that nowadays disciplines studying labour markets, political economy, health, crime, environment, etc. have exploited RD designs (see Lee and Lemieux, 2010, pp. 339–342).

⁴Cook 2008 provides an informative overview of the early phases and development of the RD design in different fields.

The increased use of RD design in empirical work has not been without corresponding advancements on the theoretical side. Hahn et al. (2001) formally stated the assumptions that were required for the identification of constant and average heterogeneous RD treatment effects. More recent theoretical work includes, but is not by any means limited to, e.g. studying cases where the running variable is discrete (Dong, 2015; Kolesár and Rothe, 2018; Lee and Card, 2008), methods for optimal bandwidth selection (Calonico et al., 2020, 2014b; Imbens and Kalyanaraman, 2012) and inclusion of additional predetermined covariates (Calonico et al., 2019). The rest of this section is primarily based on review articles such as those by van der Klaauw (2008), Imbens and Lemieux (2008), Lee and Lemieux (2010), and Cattaneo, Idrobo, and Titiunik (2018; 2019), that have been written to guide the work of applied researchers.

RD design can be framed in the regression model framework (van der Klaauw, 2008) or using the potential outcome notation that was imported from experiments to observational studies by Rubin (1974). Similar to Imbens and Lemieux (2008), we will adopt the potential outcome framework for presenting the RD design. Consider that a researcher is interested in finding out the causal effect of giving a treatment to a random sample of N units from some population, indexed by $i = 1, 2, \dots, N$, relative to not giving the treatment. Let $Y_i(0)$ denote the potential outcome, i.e. the outcome that we would observe for unit i , if the unit did not receive the active treatment. Likewise, let $Y_i(1)$ denote the potential outcome that we would observe for unit i , were the unit treated. If we use a treatment indicator T_i that equals 1 if the unit is treated and 0 if the unit is not treated, we may write the observed outcome Y_i^{obs} as

$$Y_i^{obs} = Y_i(0) + [Y_i(1) - Y_i(0)] \cdot T_i = \begin{cases} Y_i(0), & \text{if } T_i = 0, \\ Y_i(1), & \text{if } T_i = 1. \end{cases}$$

In the empirical application below, the units are individuals from a certain population, and the treatment T_i is an indicator denoting eligibility for the tax exemption. The potential outcomes $Y_i(t) \in \{0, 1\}$ denote whether the individual would have purchased an apartment under the treatment status $t \in \{0, 1\}$. Hence, $Y_i(0)$ is an indicator that receives value 1 if individual i would conduct a first-time home purchase without eligibility for the tax exemption, and 0 if individual i would not conduct first-time home purchase without the eligibility. Similarly, $Y_i(1)$ equals 1 if individual i would conduct a first-time home purchase while being eligible for the tax exemption, and 0 if i would not conduct a first-time home purchase despite being eligible for it.

The causal effect of the treatment on unit i can be defined as $Y_i(1) - Y_i(0)$, i.e.

the difference in the outcome of interest, were unit i to receive the treatment versus not receiving the treatment. For us, this is the difference in becoming a first-time homebuyer if unit i would be tax exempted versus not being tax exempted. Because of the binary nature of the potential outcomes in this particular case, the causal effect on unit i would then belong to set $\{-1, 0, 1\}$. 0 would correspond to the case where i 's first-time homebuying decision is not affected, whatsoever, by i 's tax exemption eligibility status. On the other hand, with above notation, causal effect would be 1 if i would become first-time homebuyer only if eligible for the tax exemption. Finally, -1 would correspond to the, perhaps counterintuitive case, where i would become a first-time homebuyer only without eligibility. This corresponds to the event that i would postpone or abandon the decision to become a first-time homebuyer only because (s)he is eligible for the tax exemption.

The “fundamental problem of causal inference”, contributed to Holland (1986), refers to the fact that we can never observe the same individual in both states — being eligible and not being eligible to claim the tax exemption — at the same point in time. Therefore, in order to conduct causal inference for a given unit i , it is required that we impose some additional assumptions. Often, and also in RD design, the estimand is not the causal effect for a single unit, but the average treatment effect (ATE) for some population. For example, the ATE for individuals with a given covariate value $X_i = c$, which can be written as $E[Y_i(1) - Y_i(0) | X_i = c]$.

What distinguishes RD design from other observational studies, is that we have specific auxiliary information regarding the treatment assignment mechanism. In RD designs, there is a covariate X_i , known as the running variable⁵, and a threshold c , such that the probability of receiving the treatment conditional on covariate X_i is discontinuous at that particular threshold $X_i = c$ because,

$$\lim_{x \rightarrow c-} P(T_i = 1 | X_i = x) \neq \lim_{x \rightarrow c+} P(T_i = 1 | X_i = x),$$

Alternatively, the assignment probability may be a function of a vector of covariates, and there may be multiple cutoffs where the probability in treatment assignment jumps at each cutoff. Because the simple case with a single cutoff for a single covariate corresponds to the empirical setup below, it will be the only one discussed further.

⁵There are many synonyms for the running variable: assignment, selection, ratings (van der Klaauw, 2008), treatment-determining (Imbens and Lemieux, 2008), score, index (Calonico et al., 2019), and forcing (Gelman and Imbens, 2019) variable.

4.1.1 Sharp RD design

Typically, an explicit distinction between sharp and fuzzy RD design is made. In sharp RD design, the probability of receiving treatment is a deterministic function of the running variable X_i . All units with $X_i < c$ receive the treatment, while none of the units with $X_i \geq c$ are treated. In other words, the treatment probability drops from 1 to 0 at the threshold, or cutoff c , and therefore, the treatment assignment mechanism simply equals $T_i = \mathbb{1}\{X_i < c\}$ for all i ⁶. Consider a sample of 20–60 years old persons who have never owned a share of 0.5 or above on any single-family detached house or housing company apartment. In this sample, age would be the covariate X_i which determines eligibility to the tax exemption, and the age threshold would be $c = 40$. Everyone below the age threshold, those with $X_i < 40$, but no one at 40 or above, with $X_i \geq 40$, would be eligible for the transfer tax exemption. Hence, the probability of being treated would experience a one time drop from 1 to 0, at age 40.

Contrary to this, in fuzzy RD design, the discontinuity in the treatment assignment probability does not necessarily go from zero to one, or equivalently drop from one to zero, as long as there is a discontinuity. Therefore, sharp RD design can be considered as a special case of the more general fuzzy RD design. An alternative way to frame the fuzzy RD design resembles that of a typical instrumental variable setup (Cattaneo et al., 2018; Hahn et al., 2001). One can consider that the assignment to treatment in fuzzy RD design still equals $T_i = \mathbb{1}\{X_i < c\}$, but that there is only partial compliance with the treatment assignment. Hence, in addition to units that comply with the treatment assignment, there may be units that were not assigned to the treatment but nevertheless received it, and units who were assigned to the treatment but did not comply and thus did not receive the treatment. Because of the difficulties of credibly estimating the discontinuity in transfer tax exemption eligibility at age 40 in the general population, we will restrict our attention to a specific subpopulation to whom I argue that the sharp RD design (approximately) applies. Therefore, only the sharp RD design is relevant for the upcoming empirical application, and the rest of the discussion will be devoted to it instead of the fuzzy RD.

Using the observed outcome notation and the age threshold for the tax exemption policy, the observed decision for individual i to become a first-time homebuyer in a sharp

⁶Typically, sharp RD is presented with treatment probability jumping from 0 to 1, instead of dropping from 1 to 0. Here, we consider eligibility to tax exemption as the treatment, which drops to zero at 40, so it makes sense to swap the roles around. One may equally well consider the treatment being non-eligibility, but then notation that was used above for potential outcomes would have to be adapted respectively.

RD setup can be written as:

$$Y_i^{obs} = \begin{cases} Y_i(0), & \text{if } X_i \geq 40, \\ Y_i(1), & \text{if } X_i < 40, \end{cases}$$

where X_i is the age of individual i . This means that there are only eligible observations at one side (below) and non-eligible observations at the other side (above) of the threshold age of 40. Although, there will be no observations for which $Y_i(1)$ is observed with $X_i \geq 40$, if the running variable X_i , age, is continuous, then as the sample size increases there will be observations with $Y_i(1)$ in the neighbourhood of $X_i \in [40 - \epsilon, 40)$, for arbitrary small $\epsilon > 0$. Observations with $X_i < 40$ can be used to estimate $E[Y_i(1) | X_i]$ below the threshold and, respectively, observations with $X_i \geq 40$ can be used to estimate $E[Y_i(0) | X_i]$ above the threshold. Therefore, at point $X_i = 40$ it is “almost” possible to observe both CEFs at the same time. If $E[Y_i(1) | X_i]$ is estimated based on observations with $X_i < 40$, and extrapolated for values $X_i \geq 40$, then the least amount of extrapolation is required precisely at the age threshold of $X_i = 40$.

This motivates the sharp RD estimand τ_{SRD} that is defined as the ATE at the discontinuity threshold of age 40:

$$\tau_{SRD} = E[Y_i(1) - Y_i(0) | X_i = 40]$$

Hahn et al. (2001) present the sufficient continuity assumptions required for identification. We adopt the stronger than necessary assumptions that both CEFs, $E[Y_i(0) | X_i]$ and $E[Y_i(1) | X_i]$ are continuous at the threshold $c = 40$ (Imbens and Lemieux, 2008),

$$\lim_{x \rightarrow 40-} E[Y_i(t) | X_i = x] = E[Y_i(t) | X_i = 40] = \lim_{x \rightarrow 40+} E[Y_i(t) | X_i = x],$$

for $t \in \{0, 1\}$. Intuitively, this corresponds to assuming that the mean potential outcomes are arbitrarily close to each other in arbitrarily small neighbourhoods around $X_i = 40$. If this holds, it implies that the parameter of interest is equal to the difference in the limits of the average *observed* outcomes at different sides of the threshold.

$$\lim_{x \rightarrow 40-} E[Y_i^{obs} | X_i = x] - \lim_{x \rightarrow 40+} E[Y_i^{obs} | X_i = x] = E[Y_i(1) - Y_i(0) | X_i = 40] = \tau_{SRD}$$

In other words, individuals who are just above 40 years of age are, on average, “almost” equally likely to become first-time homebuyers had they been eligible for the tax exemption, as those who we observe just below age 40. Likewise, those units just above age

40 are, on average, “almost” equally likely to become first-time homebuyers than those units below age 40 would be, were they not eligible for the tax exemption. Although the continuity of the CEFs is only required at the discontinuity threshold, it may be reasonable to assume that the CEFs are continuous also for other values of $X_i = x$ (Imbens and Lemieux, 2008). Assuming continuity only at a single point would in many settings be rather unusual and might require further substantive arguments to support why the continuity assumption is justified at this particular value but not at other values.

4.1.2 Estimation

Because the estimand in the sharp RD design is the difference between the limits of two conditional expectation functions, $E[Y_i(0) | X_i = x]$ and $E[Y_i(1) | X_i = x]$ at the boundary point $X_i = c$, estimation of τ_{SRD} requires estimating these limits in one way or another. When researchers are working with finite samples and continuous running variable X_i , there are typically only a limited number of observations available that have X_i “very close to” or at the cutoff. Therefore, there exists the need to use observations that are farther away from the threshold to estimate the CEF at, or extrapolate the CEF to, the cutoff (Cattaneo et al., 2019).

In the early RD literature, many researchers estimated these limits by fitting potentially high-order global polynomials to the data, with separate coefficients on different sides of the cutoff. An important problem with using global polynomials, however, is that while it leads to a good overall approximation of the underlying CEF, it may lead to poor performance at the boundary points that are of the utmost importance in RD design (Cattaneo et al., 2019). Gelman and Imbens (2019) discuss in detail and further demonstrate by empirical examples, why high-order global polynomials should in general be avoided in RD estimation. Their criticism consists of three main arguments. First, using high-order global polynomials in the estimation may give excessive implicit weights to those observations that are farthest away from the cutoff. In addition, these weights as well as their signs may potentially be very sensitive to the order of the polynomial, which is not an attractive feature. Second, not only the implicit weights but the point estimates themselves may be very sensitive to the order of the polynomial. Gelman and Zelizer (2015) provide examples of published research, where the conclusions are heavily dependent on particular polynomial specifications. Third, simulation exercises demonstrate that global polynomials may lead to confidence intervals that have coverage rates below their nominal level. This means that the nominal 95% confidence intervals contain the true parameter value less than 95% of the time. For these reasons, recent

papers have advised against using global, especially high-order, polynomials and instead have recommended using local polynomial — preferably linear or quadratic — regression (Cattaneo et al., 2019; Gelman and Imbens, 2019).

In local polynomial estimation, the model is not assumed to be correctly specified. Instead, it is only meant to provide accurate local approximations of the CEFs at the cutoff by only using the information on those observations that are closest to the cutoff. Using local linear regression was already recommended in the theoretical work of Hahn et al. (2001), motivated by its good boundary properties (see Imbens and Lemieux, 2008), relative to an alternative nonparametric estimation method, one-sided kernel regression. By now, local polynomial regression has become the standard way of estimating RD designs (Calonico et al., 2014b; Cattaneo et al., 2019). Local polynomial regressions use only those observations that fall within a certain range, referred to as bandwidth, h from the cutoff, i.e. observations with $X_i \in [c - h, c + h]$. Furthermore, local polynomial regressions typically include weights that are determined by a kernel function $K\left(\frac{X_i - c}{h}\right)$, where each observation is weighted as a function of its distance from the cutoff. This allows researchers to assign more weight to those observations that are closer to the threshold relative to those that are farther off, which may lead to more accurate boundary approximations. For example, Cattaneo et al. (2019) recommend using triangular kernel $K(u) = (1 - |u|) \cdot \mathbb{1}\{|u| \leq 1\}$, which leads to a point estimator with some optimal properties when used in conjunction with mean squared error (MSE) optimal bandwidth.⁷

On the other hand, point estimates tend typically not to be very sensitive to kernel choice (Cattaneo et al., 2019; Imbens and Lemieux, 2008). Imbens and Lemieux (2008) recommend using uniform weights and assessing sensitivity to bandwidth choice, because sensitivity to kernel choice may indicate sensitivity to the bandwidth. Local polynomial regression with uniform kernel, $K(u) = \mathbb{1}\{|u| \leq 1\}$, corresponds to a standard OLS regression, where we have restricted the sample only to those observations that fall within bandwidth h from the cutoff.

Contrary to global polynomial regression, local polynomial regression does not give excessive weights — sensitive to polynomial order — to observations far away, because all observations more than the bandwidth away from the threshold receive zero weights. The estimates from local polynomial regressions are generally also less sensitive to the order of the polynomial. Furthermore, simulations suggest that low-order local polynomial regressions have typically smaller standard errors than high-order global polynomials,

⁷For more discussion on MSE-optimal bandwidth selection, see e.g. Imbens and Kalyanaraman (2012) and Calonico et al. (2014b).

and the coverage of the confidence intervals that are based on local methods have coverage rates closer to their nominal level than those based on global methods (Gelman and Imbens, 2019). On the other hand, Lee and Lemieux (2010) consider that nonparametric local polynomial methods should rather be seen as a complement instead of a substitute to parametric global polynomial estimation, and the most credible estimates are those that are stable across alternative specifications. Gelman and Imbens (2019) also point out that using a high-order global polynomial can be a perfectly sensible thing to do if there is a reason to believe that it leads to accurate approximations of the CEFs at the boundary. This, however, may not be the typical case in empirical work.

Recently, an alternative to the continuity-based RD framework was formalized by Cattaneo, Frandsen, and Titiunik (2015). In RD studies, where the estimation is based on assuming continuity of CEFs at the cutoff, it is not uncommon to refer to the treatment assignment as being as good as randomly assigned within a small bandwidth around the threshold. Motivated by this, Cattaneo et al. (2015) formalize the assumptions required to interpret RD design as a local randomized experiment. First, one selects a small window around the threshold, within which the local randomization assumption is considered credible, which allows conducting exact finite-sample inference as opposed to inference based on large-sample approximations. This approach has been suggested to be a complement and a form of robustness check for the results in continuity based framework (Cattaneo et al., 2015), and a potentially better and more credible framework for studying RD designs where the running variable is discrete with only a small number of mass points (Cattaneo et al., 2018).

4.1.3 Discrete running variable in RD design

Up until the last paragraph, we have implicitly assumed that the running variable is continuous. However, in many real-world settings, the running variable may either be discrete, or it is continuous, but the researcher is only able to observe a discretised version of it, e.g. because of rounding. This was also the case in the first RD paper by Campbell and Thistlethwaite (1960), where students' aptitude test scores were treated as belonging to different interval bins. The discrete nature of the running variable introduces some further complications into the estimation when there exist only a few mass points in the support of the running variable.

When the running variable is discrete, even as the sample size tends to infinity, there will never be observations that are closer to the cutoff than the two closest mass points in the support of the running variable, and the treatment effect cannot be nonparamet-

rically identified (Lee and Card, 2008). Therefore, there are never observations that are “just” above and below the threshold, and the CEFs have to be extrapolated to the cutoff. However, even if the running variable was continuous, in practice researchers always deal with finite samples. Therefore, there are never observations that are arbitrarily close to the cutoff, and some extrapolation is always present (Lee and Lemieux, 2010). For this reason, some researchers (Cattaneo et al., 2018; Kolesár and Rothe, 2018; Lee and Lemieux, 2010) do not see the discreteness of the running variable per se preventing the use of local polynomial methods and to require using global polynomial methods. What matters the most is whether there are enough mass points close to the threshold (Cattaneo et al., 2018; Kolesár and Rothe, 2018). The problem with using a local polynomial and discrete running variable arises if the CEF differs from the estimated polynomial and estimation uses a bandwidth that is too large in the sense that the bias induced by this misspecification is not negligible (Kolesár and Rothe, 2018; Lee and Lemieux, 2010). If the running variable is continuous, then with enough observations, one can always reduce the bandwidth to decrease the misspecification bias, but the discrete running variable limits how much the bandwidth can be narrowed down.

Dong (2015) demonstrates that discretization caused by rounding the running variable would still yield biased estimates if the functional forms of the CEFs were truly known, and she shows how auxiliary information on the distribution of the rounding error can be used to correct for this bias. For example, if the model is correctly specified but age is measured in years, the bias can be reduced if we have information on how the birthdays are distributed in the population that the sample was drawn from (Dong, 2015). The key takeaway from Dong (2015) is that what matters for the bias is how the slope and higher derivatives of the outcome behave as a function of the running variable at the cutoff. In case they do not change at the cutoff, then the bias induced by the rounded running variable is zero, but otherwise, the estimate may either be biased upwards or downwards.

Generally, the functional form is not known, and in an influential paper on RD with discrete running variable Lee and Card (2008) model the deviation of the estimated specification from the true CEF as a random error. Because this error is the same for all observations that have the same value of the running variable, they recommend that one should at bare minimum use cluster-robust standard errors. If the specification error is identical in size and magnitude for both CEFs, i.e. for a given value of $X_i = x$, the specification error is the same when estimating $E[Y_i(0) | X_i = x]$ and $E[Y_i(1) | X_i = x]$, then using standard errors that are clustered at the level of the running variable is enough. However, if the specification errors are not identical, but independent, one

should further inflate the standard errors by accounting for the additional variance that is induced by these specification errors, in a manner that is described in more detail in Lee and Card (2008).

Following the advice of Lee and Card (2008), many papers (e.g. Imbens and Lemieux, 2008; Lee and Lemieux, 2010; van der Klaauw, 2008) recommend using cluster-robust standard errors in RD design with a discrete running variable. This became the “conventional wisdom” and was widely adopted in applied studies (Kolesár and Rothe, 2018). Contrary to this recommendation, in a recent study, in addition to critiquing the random error approach of Lee and Card (2008), Kolesár and Rothe (2018) provide theoretical, simulation, and empirical evidence why the use of cluster-robust standard errors might want to be avoided in RD settings. They show that when the bandwidth, hence misspecification, is small to moderate and the number of clusters is small, using cluster-robust standard errors may lead to even smaller standard errors than the typical heteroskedasticity-robust standard errors. When misspecification is present, this is the opposite of what is required for the confidence interval to reach its nominal level. Hence, using cluster-robust standard errors may lead to confidence intervals that have coverage rates substantially below the nominal level. Furthermore, Kolesár and Rothe (2018) demonstrate that if both the bandwidth and the number of clusters are large, the cluster-robust standard errors are larger than the heteroscedasticity-robust standard errors, but even in this case, the confidence intervals may lead to substantial under coverage. Instead, they recommend that in this situation it is better to narrow down the bandwidth to reduce the amount of misspecification and use confidence intervals that are based on heteroskedasticity-robust standard errors. Alternatively, one may construct confidence intervals by using two alternative approaches that rely on bounding the second derivative or bounding the misspecification error, yielding better coverage properties than the cluster-robust standard errors (Kolesár and Rothe, 2018).

Although, age at the moment of apartment purchase is a continuous variable, in the data that we use for the empirical exercise, we are only able to observe age measured in years at the end of the calendar year. The running variable will therefore be discrete, and there are only a limited number of mass points close to the age threshold of 40 — especially when using narrow bandwidths in order to reduce the misspecification error. Using age variable that is measured in years in RD studies is not unusual, as shown by Kolesár and Rothe (2018) who reanalyse two previously published studies with age as the running variable and the list of RD designs by Lee and Lemieux (2010, pp. 339–342). However, because of the small number of mass points, I will follow the advice of Kolesár and Rothe (2018) to use heteroskedasticity-robust standard errors instead of clustering

at the age variable.

The estimation will be conducted using the continuity-based framework instead of the local randomization framework of Cattaneo et al. (2015). Cattaneo et al. (2015) state that if the local randomization assumption with a discrete running variable is credible at any window around the threshold for which there are observations, it has to hold for at least for the two closest mass points around the discontinuity. Because of data issues that are discussed in detail in the next section, observations at age 40 will be excluded, and thus the local randomization approach would correspond to comparing the outcomes of those aged 39 and 41 with each other. First, it does not seem reasonable to assume that age is as good as randomly assigned even at this smallest possible window between those aged 39 and 41. Second, there is the additional concern that some individuals may have shifted their purchase decisions ahead in time to benefit from the tax exemption, which is why we would like to assess the sensitivity to observations that are closest to the threshold by using a donut hole approach. This means excluding observations at ages 39 and 41 from the specifications that are estimated. Finally, I will study the sensitivity to bandwidth choice separately. With triangular kernel and bandwidth 2, weight is only given to observations at ages 39 and 41, which effectively compares the means of these two groups. This is similar to the local randomization approach, although the inference will still be based on large-sample approximations.

4.2 Data

The data comes from two primary sources: Finnish Tax Administration and Statistics Finland. I use individual-level data provided by the Finnish Tax Administration on annual housing company shareholdings. This data consists of all housing company shareholdings from the years 2005–2016 in Finland. The data contains information on the size of the apartment, year of purchase, purchase price, the outstanding debt of the housing company that the owner is responsible for, ownership share, an indicator for whether transfer tax exemption was claimed, and encrypted personal identifier. The data by Tax Administration also contains annual microdata from 2002–2015 on the outstanding mortgage debt at the end of the year and mortgage interest payments during the year.

Statistics Finland provided access to the FOLK basic data module⁸, which contains detailed annual microdata on all permanent residents in Finland, covering the years 1987–2019. It contains information on key socio-economic and -demographic variables,

⁸A detailed description of the FOLK basic data module can be found at https://taika.stat.fi/en/aaineistokuvaus.html#!?dataid=FOLK_19872019_jua_perus20_002.xml.

e.g. gender, education, marital status and family structure, employment, disposable income, and municipality. It also contains a few housing-related variables like the number of rooms (excluding kitchen), tenure status, building structure, and standard of equipment in the dwelling. All the variables in the FOLK basic data module are measured on the last day of the year. When background covariates are reported, the lagged values from the end of the year $t - 1$ are used to make sure that the values are predetermined. Statistics Finland also provided keys, which allows linking the housing company shareholdings data with the FOLK basic data module.

4.2.1 Data limitations

4.2.1.1 Linking observations from the two data sources

I will restrict the attention only to the years 2006–2015 because of data issues. For some reason, the year 2005 contains 41% fewer transactions than the average in the years 2006–2016, and it is the only year that does not contain data on ownership shares which is needed for the proxy discussed below. Year 2016 will be dropped from the analysis because 95.72% of the total 21,100 tax-exempted apartment purchases were encrypted with personal identifiers that did not have a counterpart in the FOLK data. In other words, linking the two data sources failed for the year 2016.

However, linking observations from these two data sources was not perfect in other years either. Appendix Table A.1 presents the annual frequencies and shares of those apartment purchases in which personal identifiers in Tax Administration’s data could not be linked with personal identifiers used in FOLK data. Panel A presents the results for all apartment purchase transactions and panel B only for those transactions that were tax exempted. In 2006, 33.54% of the tax-exempted transactions conducted by households suffered from the same issue. This share monotonically decreased to 7.19% in 2010. In the years 2011–2015 at most 1.67% of tax-exempted purchases could not be linked with personal identifiers used in the FOLK data, whereas for years 2013 and 2015 all tax-exempted transactions conducted by households could be merged with a working personal identifier. When we look at all transactions, instead of just tax-exempted transactions, the qualitative conclusions remain the same. Quantitatively the share of transactions without matching identifier in the FOLK data are generally smaller varying between 4.89% and 20.85% in the years 2006–2010. In the years 2011–2015 all but at most 1.84% of the transactions contained a working personal identifier.⁹

⁹For apartment buyers with working personal identifier, the data failed to be merged for 1.05%–1.37% annually, with no clear pattern over time. Unless the personal identifiers were erroneous, this means that

It seems substantial that for some years more than a fifth of the tax-exempted apartment purchases could not be traced back to the buyer. It can be argued that only years in which the linking failure was negligible should be used in the analysis. Including all years will lead to smaller estimated standard errors, but if the observations are not missing at random, and there are some systematic differences between those whom the linking failed and those who remain in the sample, the estimates might be biased. At first, the missing observations will be treated under the missing at random assumption, and then robustness will be assessed by conducting the estimation using only years 2011–2015.

4.2.1.2 Population of interest and tax exemption eligibility

As discussed in the previous section, in sharp RD design the probability of treatment assignment changes from 0 to 1, or vice versa, at the threshold, whereas for fuzzy RD design, the discontinuity at the threshold needs to be estimated. Hence, in order to study how the eligibility to transfer tax exemption affects first-time homebuying at age 40, we have to be able to estimate how much does the eligibility change at age 40. Clearly, the probability of being eligible to claim the tax exemption at 40 and above is zero due to the eligibility criteria. However, some individuals below age 40 have already claimed the tax exemption at a younger age, and therefore, in the general population, the mean tax eligibility conditional on age does not exhibit a drop from one to zero. Hence, with the general population, the RD setting in question would correspond to the fuzzy design. The problem with studying the effects of the eligibility for the transfer tax exemption in the general population is that the share of eligible 18 to 39-year-olds is not known — and to my understanding cannot credibly be estimated using the data at hand. The main issue is that the Tax Administration’s data only contains information on the ownership shares starting from the year 2006. Before this, we cannot know whether someone observed living in a housing company apartment has owned an ownership share of 0.5 or above, and hence we cannot know the eligibility status. Likewise, the eligibility status of those who are observed living in single-family detached houses remains a mystery, unless they have held an ownership share of 0.5 or above of a housing company share in the post-2006 period.

Acknowledging the problems related to the eligibility status when studying the effects on the whole general population leads me to restrict the population of interest to a subpopulation whose eligibility status discontinuity, I argue, we are more certain of. Specifically, to those 20–60 years old who have not lived in owner-occupied dwellings

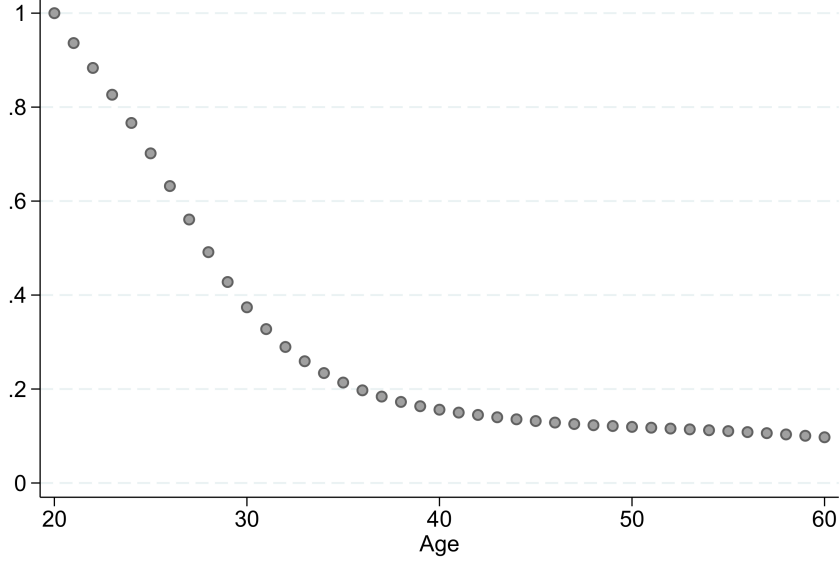
all apartment buyers were not observed in the FOLK data, i.e. not permanently residing in Finland at the of the purchase year.

Table 1: Descriptive statistics for never-owner-occupiers.

Variable	Mean	Std dev.	Minimum	Maximum	Observations
Age	31.5	11.1	20	60	8,364,077
Female	.448	.497	0	1	8,364,077
First-time homebuyer	.025	.155	0	1	8,364,077
Education					
Upper secondary	.566	.496	0	1	8,364,077
Short-cycle tertiary	.029	.167	0	1	8,364,077
Bachelor	.081	.273	0	1	8,364,077
Master	.045	.207	0	1	8,364,077
Doctoral	.003	.050	0	1	8,364,077
Unknown	.275	.446	0	1	8,364,077
Language					
Finnish	.832	.374	0	1	8,364,077
Swedish	.043	.203	0	1	8,364,077
Other	.125	.331	0	1	8,364,077
Main activity					
Employed	.542	.498	0	1	8,364,077
Unemployed	.117	.322	0	1	8,364,077
Student	.161	.368	0	1	8,364,077
Retired	.061	.239	0	1	8,364,077
Household size	2.40	1.381	1	9	7,875,841
Family status					
Head	.194	.395	0	1	8,364,077
Spouse	.163	.370	0	1	8,364,077
Child	.232	.422	0	1	8,364,077
Not part of a family	.362	.481	0	1	8,364,077
Disposable income	13,727	9,176	0	103,600	8,364,077
Outstanding mortgage debt	2,810	16,612	0	4,673,034	8,364,077
Owens a car	0.350	0.477	0	1	8,364,077
Municipality of residence					
Urban	.777	.416	0	1	8,364,077
Semi-urban	.100	.300	0	1	8,364,077
Rural	.104	.305	0	1	8,364,077
Number of rooms	2.63	1.37	1	20	7,797,322
Tenure status					
Owner-occupied dwellings					
Owens the house	.150	0.357	0	1	8,364,077
Owens the shares	.047	.211	0	1	8,364,077
Rental apartment	.687	.464	0	1	8,364,077
Other	.046	.210	0	1	8,364,077
Building structure					
Detached house	.197	.398	0	1	8,364,077
Terraced house	.108	.310	0	1	8,364,077
Multi-storey building	.608	.488	0	1	8,364,077

Notes: Data is from 2006–2015. First-time homebuyer status is measured by the proxy. The percentages in subcategories do not sum to 0 due to rounding and omitted subcategories with small numbers of individuals or those with missing values. These are presented only if they consist a substantial share. FOLK basic data module truncates household size at 9 and the maximum disposable income is presented as the median of the highest percentile. In addition, number of rooms has been truncated at 20 and the maximum of outstanding mortgage debt is calculated as the mean of the highest percentile of those with non-zero values. For details on FOLK variables see https://taika.stat.fi/en/aineistokuvaus.html#!?dataid=FOLK_19872019_jua_perus20_002.xml.

Figure 1: Share of never-owner-occupiers in the general population by age.



while being classified by their family status as “not part of a family” or as “heads or spouses of a family”, after age 20 or since the beginning of the data. Pooled over the years 2006–2016 there are 8,364,077 such observations. The restriction on the family status is made so that we do not exclude young individuals who live in the same household with their parents¹⁰, regardless of the tenure status of the dwelling, as it is most likely the parents who own the dwelling. The original goal of the transfer tax exemption was to provide financial support for young households to start their own lives in owner-occupied dwellings. Therefore, focusing on this population might be more relevant than on the general population, if we want to study whether the policy has reached its goals.¹¹ Figure 1 shows the share of individuals in the general population by age that belong to this group (henceforth never-owner-occupiers¹²). Table 1 presents the descriptive statistics for this group.

According to Table 1, this group largely consists of younger individuals: the mean age is 31.5, and this is also reflected in Figure 1, which shows the shares at each age. Eventually every 20 years old starts by being part of this group, but at age 30 only less than 40% remain. The share decreases to 20% at age 36 and 16% at age 40. This also

¹⁰Neither are the rare cases with unknown family status excluded from the sample, regardless of the tenure status.

¹¹On the other hand, it is unclear whether 40-year-olds can be considered as young households that the legislators were primarily concerned for.

¹²This name is chosen for brevity and it is a slight mischaracterization as it only refers to past tenure status. Some “never-owner-occupiers” indeed become homeowners.

explains the rather large share of children: it is more typical for the youngest individuals to live among their parents. The majority of the group, 56.6%, have upper secondary education as their highest completed education level. Partly, this reflects the large share of young individuals. It is also consistent with the fact that individuals with tertiary education have on average higher earnings and financing a mortgage may therefore be easier and they may become ineligible relatively fast at a younger age than those with upper secondary education. For a rather substantial portion 27.5% there is no data on their highest completed education. This group may partly consist of those without upper secondary education or higher because FOLK did not record education levels below upper secondary level. Of the general population in 2015, 15.6% between the ages of 20 and 59 had not completed upper secondary level education or higher, which suggests that their share may well be overrepresented in our population of interest¹³. This would also be consistent with the easier access to homeownership among higher educated individuals.

Disposable income in Table 1 refers to personal, not household, disposable income. Disposable income of €13,727 among never-owner-occupiers is relatively low compared to the mean disposable income of €28,986 in the general population of those aged between 18 and 64 in year 2015. Disposable income has rather large standard deviation, which is typical in income distributions: disposable income cannot be negative, and the distribution tends to be skewed to the right. Clearly, younger individuals have usually also lower disposable income. Males and those speaking other than Finnish and Swedish are also disproportionately represented among never-owner-occupiers relative to the general population. For comparison, the share of other than Finnish and Swedish speaking individuals was 6% in the whole population in year 2015 but 12.5% among never-owner-occupiers. Otherwise, Table 1 reveals what is to be expected with the restrictions we impose to identify eligible individuals: most of the individuals live in rental multi-storey apartments. Overall, the descriptive statistics show that those who have not previously lived in owner-occupied units after age 20 independently of their parents, are individuals to whom homeownership may be considered on average harder to attain.

For this subpopulation of individuals, who after turning 20 years old have never lived in owner-occupied apartments independent of their parents, I assume that everyone under the age of 40 is eligible to claim the transfer tax exemption, and no one else is. Under this assumption, the setting corresponds to the sharp RD design. There are some

¹³This and the rest of the comparisons in the next paragraph are based on Statistics Finland's PxWeb database (<http://pxnet2.stat.fi/PXWeb/pxweb/fi/StatFin/>) because at the moment of writing this I no longer had access to the FOLK database.

reasons why this assumption may fail to hold in reality.

First, tenure status is measured at the end of the year. This makes it possible to claim the tax exemption for a first-time home purchase that happens at the beginning of the year and to move out of the unit to a rental apartment or back to parents' household by the end of the same year. If the person does not live in the unit at the end of at least one calendar year, the move goes unrecorded, and if the person does not become owner-occupier again before age 40, the discontinuity in the eligibility share at the cutoff does not drop from 1 to zero. Second, some individuals who would have otherwise been eligible for the tax exemption may have purchased investment apartments before the year 2006 — for which there is no data of — but have themselves lived e.g. in rental apartments. Such persons are not eligible, although they would still falsely be included in our sample. Assuming, of course, that their ownership share in any investment apartment reached 0.5 or above. Third, basing the eligibility status on tenure history starting only from the year the individual turns 20 years old means that tax exemption may have been claimed already when the individual was 18–19 years old. For this to bias the estimate at 40, it would require that the individual does not live independently in owner-occupied units at the end of calendar years for almost two decades. On the other hand, there are not many 18–19 years old buying housing company dwellings in the first place: only 0.13% and 0.36% of the age groups, and such individuals likely live in owner-occupied units also in the following years. For example, in 2006–2015 of all the 18 to 19-year-old apartment buyers with ownership shares of 0.5 or above, 92.18% were not considered eligible anymore by our standards at age 25. Furthermore, this share may also contain biases arising from the two previous sources, and it is credible to assume that the bias in eligibility status will keep on decreasing as the age increases.

In addition to the above, the composition in terms of tenure history may vary in age. Some individuals may have been owner-occupiers before the data begins, but never in those years for which we have data available. Consider e.g. a person who was 42 years old at the end of 2006 and had lived in an owner-occupied unit at age 22 in the year 1986. Had this person lived e.g. in rental apartments from the end of 1987 until the end of 2006, the composition of 42-year-olds would differ from the younger age groups in terms of their tenure history. Albeit, this person too, would have eventually been eligible to the transfer tax exemption if the previous dwelling was sold off before the year 1990.¹⁴ Above the 40 years threshold, the younger age groups are less likely to differ in tenure history from those below the threshold, because to bias the composition, the younger they would have to had been at the time of their initial apartment purchase.

¹⁴See chapter 2.

On the other hand, below the 40 years threshold, the compositions in tenure histories do not vary in age, because we are only considering tenure and family statuses starting from the year that the individual turns 20 years old. Individuals who were 39 years old at the end of 2006 were 20 years old at the end of 1987, and hence for them, and for everyone who turned 39 years old in 2006–2015, we are guaranteed to have the full tenure status history at the end of the year since they turned 20. This is the reason why the sample restriction criterion is only applied to those at or above age 20 instead of age 18. Alternatively, in order to have the same tenure status history for all under 40-year-olds, we could start tracking tenure status history from age 18, but then we would have to focus only on the years 2008–2015. At least two important questions remain: whether the sources of potential bias outlined above are common, and which way are they likely to bias our results.

We do not have data available to further study whether it is common for individuals to purchase and move in and out of the apartment within a calendar year, and not to live in owner-occupied units in the following years. If someone claims the transfer tax exemption requiring permanent residency of six months in the purchased dwelling, it seems likely that the individual will reside there at the end of the same calendar year. However, if the individual decides to move away from the owner-occupied unit, it is more likely that they will move to another owner-occupied unit than to a rental apartment (Eerola et al., 2019).

Likewise, based on the available data, it is not possible to tell how common it is for never-owner-occupiers at the neighbourhood of age 40 to have held ownership shares of 0.5 or above in investment apartments. Non-taxation of imputed rental income at least gives financial incentives to owner-occupancy and living in owner-occupied units is more attainable for those who own investment apartments than those who do not. For the third source of bias — starting to follow tenures only at age 20 — it was already argued based on data from years 2006–2015 that it is unlikely to be a major concern.

If bias from these sources exists at age 40, it means that the share of individuals who are eligible to claim the tax exemption does not in reality drop from 1 to zero. Hence, our sample would include individuals who do not face policy-induced changes in financial incentives to purchase a home. Therefore, the observed effect, if any, is only driven by those who are in reality affected by losing their eligibility at age 40. This means that the estimate would be biased towards zero.

Finally, there is the potential bias in the composition of tenure status histories by those above age 40 who we do not observe in their early 20's. What is relevant is how those who are misclassified as having never lived in owner-occupied units after age 20 but

before the year 1987 differ in the probability to become first-time homebuyers from those who are correctly classified, despite the data limitations. If the misclassified individuals are more prone to become first-time homebuyers at ages above 40, this would cause downward bias, because the true effect is dampened by the differences in the tenure status history composition below and above the threshold. Similar to the 18 to 19-year-old apartment buyers, assuming that most individuals who lived in owner-occupied units in their early 20's before the year 1987, also lived in owner-occupied units at least sometime in the years 1987–2005, bias from this source is not likely to be severe. In addition, the potential bias is dependent on the estimator that is used. The local regression estimator with bandwidth 5 and triangular kernel effectively uses data above the threshold only from those units who are between 41–44 years old. These estimates would not suffer from compositional bias after the year 2010 because, in 2011–2015, all 44 years of age were 20 at the end of 1987. Hence, their whole tenure status history after turning 20 is observed, and only years 2006–2010 would potentially cause bias.

To summarize, potential sources of bias do exist, and it is good to be aware of how they may affect the estimates. It is plausible — if not certain — that even in this subpopulation the true discontinuity in transfer tax exemption eligibility does not precisely drop from one to zero. Also, in some years individuals above age 40 might differ in their tenure status histories from those below age 40 due to data availability. However, I have tried to argue that instead of inflating the RD estimate, these would bias the estimate towards zero, and instead of being the norm, they may be considered as anomalies. Besides the attempt to reduce misspecification bias by narrowing down the bandwidth used in estimation, these data limitations also provide some additional rationale to prefer estimates that are based on specifications with narrower bandwidths.

4.2.1.3 Proxy for first-time home purchase

The Tax Administration data contains an indicator for those transactions in which transfer tax exemption was claimed — transactions conducted by first-time homebuyers aged 18–39.¹⁵ There is, however, no pre-existing variable that reveals which transactions by 40-year-olds or older were conducted by individuals who failed to satisfy the eligibility criteria only because their age exceeded the 40-year age threshold. Because the primary outcome is being a first-time homebuyer, it is necessary to identify such purchases at both sides of the threshold if we are to analyse the setting as a RD design. For this purpose, I constructed and experimented with different proxy variables that indicated being

¹⁵In some transactions, the buyer appears to receive the tax exemption even if the buyer is clearly outside the age range of 18–39. I consider these to be misclassifications.

Table 2: Joint distribution of the proxy and tax exemption for ages 20–39.

Age bins	Proxy=0 Exempted=0	Proxy=1 Exempted=0	Proxy=0 Exempted=1	Proxy=1 Exempted=1	# Apt. purchases
20–24:	8.25	5.22	0.18	86.35	57,885
25–29:	8.99	4.63	0.15	86.23	91,890
30–34:	8.58	5.96	0.20	85.26	43,125
35–39:	9.77	9.77	0.28	82.20	15,680
20–39:	8.61	5.46	0.18	85.76	208,580

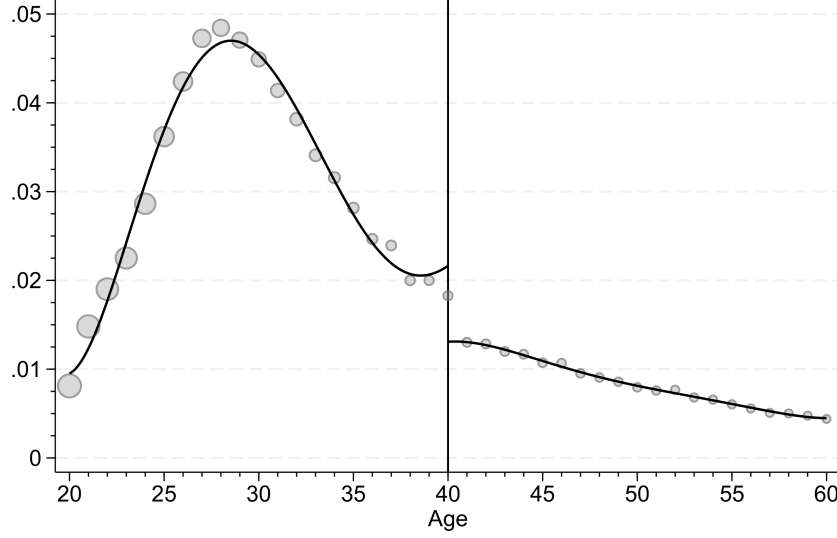
Notes: The table is restricted only to those individuals who purchased an apartment, because both proxy and tax-exemption indicator are zero for those 20–39-year-olds who did not purchase apartments. Without conditioning on apartment purchase, the proxy would classify 99.82% correctly, but obviously this is driven by the small fraction of individuals who purchase apartments in the first place.

a first-time homebuyer. The fit of the different proxies was assessed by comparing their relative performance to the tax exemption indicator among those never-owner-occupiers aged 20–39 who purchased housing company dwellings. Underlying the validity of this approach is the assumption that the proxy performs similarly just below age 40 as it does just above age 40, although there is no way for us to ascertain the performance at and above the threshold. Under this assumption, the possible discontinuity in the outcome variable at the threshold is not driven by the proxy itself.

It turns out that the best overall fit is given by a proxy that equals one for individual i in year t , if an apartment was purchased in a year that fulfills a simple condition: year t is the first year in the post-2005 period when the person is recorded as having owned an ownership share of 0.5 or above of a housing company dwelling. This proxy is simple and intuitively one would expect it to have bad performance in the general population, which it indeed does relative to alternatives. However, it outperforms all alternative proxies among the group of never-owner-occupiers.

The proxy fits are presented in Table 2 for never-owner-occupiers aged 20–39 using five-year bins. Table 2 reveals that never-owner-occupiers aged 20–39 conducted a total of 208,580 apartment purchases in 2006–2015, and 85.94% of these were exempted from transfer taxation. A substantial portion of these, 44%, were conducted by 25–29-year-olds. Additionally, Table 2 makes visible the two sources of misclassification that take place when we use proxies. Some transactions are classified as first-time home purchases by the proxy, although in reality, they were not. The share of these cases is shown in the third column. On the other hand, not all first-time home purchases are captured by the proxy, as shown by the fourth column. In this population, the proxy performs relatively similarly among 20–34-year-olds and slightly worse among 35–39-year-olds.

Figure 2: Shares of first-time homebuyers among never-owner-occupiers by age.



Notes: First-time purchases as measured by the proxy. A 5th order polynomial is calculated excluding those at age 40 and fitted separately to both sides of the threshold. Size of the marker reflects the relative size of each age group in the subpopulation. The figure demonstrates how the fifth order polynomial does a good job approximating the CEF on average, but has bad boundary performance, especially on the left side of the threshold (see Gelman and Imbens, 2019).

This is unfortunate because for RD design we would hope to see the best performance just below age 40, but the differences in overall performance are not large. The proxy seems to do extremely well in capturing almost all transactions that claimed transfer tax exemption. This comes with the cost that, especially among buyers aged 35–39, the proxy classifies more cases as first-time home purchases than it should. The table 2 also reveals that overall, in our subpopulation, receiving the tax exemption is captured by 99.8% of the time by the proxy, whereas not receiving the tax exemption is captured by 61% of the time. Fortunately, 85.94% of the apartment purchases among 20–39-year-olds were tax-exempted, which the proxy does a better job at capturing. Appendix Table A.2 presents these same results for the years 2011–2015, when there were hardly any apartment purchases with missing encrypted personal identifiers, and these results are largely the same as here.

Figure 2 presents the shares of first-time homebuyers by age among the never-owner-occupiers as measured by the proxy. In this population, the share of first-time home purchases varies between 0.5% and 5%, with 28-year-olds being the most prone to buy a first-time home. The shape of the curve resembles a log-normal distribution with a slight level shift downwards taking place between ages 40 and 41. Based on this figure

alone, it would seem that the discontinuity in first-time home purchases happens at age 41 instead of 40. This would be inconsistent with our hypothesis according to which the end of transfer tax exemption may lead to fewer transactions starting already at age 40. Next, we discuss why this is not the case.

4.2.1.4 Mismeasurement in the running variable

In addition to the difficulties of not being able to directly observe eligibility status for the transfer tax exemption and to know which transactions failed to satisfy eligibility criteria only because of age restrictions, the running variable — the age at the moment of housing company share purchase — suffers from mismeasurement error. Although we know the exact date when an apartment purchase has taken place and when the ownership was transferred to the buyer, the age variable in FOLK is measured in years on the last day of the calendar year. Because of the lack of access to the exact date of birth, or even the birth month, we are unfortunately unable to precisely determine the age of the buyer at the moment when the transaction was conducted.

More specifically, when we observe that an individual has purchased a housing company dwelling in some year, we cannot tell whether or not this transaction took place before or after the person’s birthday had passed. Mismeasurement of this sort is especially problematic for those observations where the buyer is 40 years at the end of the purchase year. We cannot be sure whether such buyers were 39 or 40 years old at the moment of purchase, even though this is what potentially — and with the restricted subpopulation, we assume, single-handedly — determines their eligibility to the tax exemption. At extreme, consider two individuals: one who bought housing company dwelling on the first day of the year while having 40th birthday at the last day of the year, and another who conducted the purchase on the last day of the year while having had 40th birthday in the first day of the same year. In our data, they would both be classified as 40 years old on the last day of the year they bought their apartments, although, in reality, the former was 39 years and 1 day old, while the latter was 40 years and 364 days old at the moment of purchase.

This problem is well illustrated by the fact that in the general population, according to the Tax Administration’s tax exemption indicator, 20.55% of 39-year-old apartment buyers claim the tax exemption, whereas the percentage of claimants remains at 12.43% among those classified as 40-year-old buyers. Similarly, among never-owner-occupiers, 52.38% of 40-year-old buyers claim the tax exemption, whereas the percentage is always above 80% for younger age groups. This suggests that a sizable fraction of purchases that

are marked to have happened at age 40 did take place when the individual was still 39 years old, and hence eligible for the tax exemption. Assuming that the joint distribution of our proxy and actual first-time homebuyers¹⁶ would have remained constant from age 39 to age 40, or,

$$P(\textit{First time} = j, \textit{Proxy} = i \mid \textit{Age} = 40) = P(\textit{First time} = j, \textit{Proxy} = i \mid \textit{Age} = 39),$$

for $i, j = 0, 1$, we find that 80.6% of those at age 40 would have been eligible for the tax exemption if the age cutoff did not exist. Under this assumption, 61.4% of all first-time homebuyers who we observe to be 40 years old at the end of the purchase year, would have been 39 years old at the moment of purchase.

Due to this mismeasurement, those aged 40 at the end of the purchase year contain both treated (eligible) and control (ineligible) individuals. Because we cannot distinguish between them, the analysis is conducted by excluding these observations. Below the cutoff, we will only use observations up until age 39 and above the cutoff, we will only use observations starting from age 41.¹⁷ This is not an unusual choice in the RD literature, e.g. Dong (2015) excludes units at cutoff age of 60 years because the group consists of both treated and control units, which corresponds to our setting. Likewise, Ferreira (2010) estimates a similar specification as a robustness check for the potential mismeasurement of age at the threshold.

¹⁶Measured by the tax-exemption indicator for 18–39-year-olds in Tax Administration’s data.

¹⁷This is why in Figure 2 the 5th order polynomial was fitted without 40-year-olds for both sides of the threshold instead of including them on the right side.

5 Results

This chapter presents the results of the empirical analysis. Section 5.1 presents the RD results from different specifications. Next, section 5.2 studies the sensitivity of the preferred specifications that are based on low-order local polynomial regressions with triangular kernel. The findings are presented graphically and formally by tables and figures, some of which can be found in the Appendix. In addition to the RD design, section 5.4 presents the linear regression results on how prices differ between dwelling purchases that are tax-exempted and those that are not. The regression analysis starts with a very simple specification to which year- and municipality-dummies, their interactions, and covariates of the apartment buyers are cumulatively introduced.

5.1 RD estimates

Estimation in this section is conducted using Stata package “rdrobust” (Calonico et al., 2017, 2014a). Table 3 presents RD estimates for seven different specifications with varying polynomial order, bandwidth, and kernel, to assess whether the discontinuity can be considered robust as suggested by van der Klaauw (2008). In Table 3 both bandwidth and polynomial order may change between the specifications. Below we will study in detail how bandwidth selection affects the main specification of local linear regression with triangular kernel. We also repeat this for the local quadratic specification. The estimates in Table 3 are reported with their 95% confidence intervals. The confidence intervals are based on HC0 heteroscedasticity-consistent standard errors, following the recommendation by Kolesár and Rothe (2018) that we discussed in section 4.1.3. These different specifications are presented graphically in Appendix Figure A.1.

The specification in column (1) in Table 3 uses all observations between ages 20 and 60. This fifth-order polynomial with uniform kernel corresponds to the same specification that is illustrated in Figure 2. The poor boundary performance of high-order global polynomials (Gelman and Imbens, 2019) is reflected in Figure 2 by the upward slope just below the threshold. This also is reflected in the point estimates in Table 3. All the other specifications with smaller bandwidths and lower-order polynomials have smaller point estimates. Columns (2)–(4) report the estimates for second-order and third-order polynomials. These specifications suggest that the end of tax exemption at age 40 reduces the probability of first-time home purchase by 0.47–0.63 percentage points. Columns (5)–(7) report local linear regression estimates with bandwidths 3 and 5. Generally, these specifications have smaller estimates than those with higher-order polynomials:

Table 3: RD estimates for the years 2006–2015.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Estimate	.0086	.0057	.0047	.0063	.0032	.0042	.0068
95% CI	[.0062, .0108]	[.0035, .0078]	[.0030, .0060]	[.0032, .0094]	[.0021, .0043]	[.0028, .0056]	[.0044, .0092]
p-value	.0000	.0000	.0001	.0001	.0000	.0000	.0000
Bandwidth	20	10	7	5	5	5	3
Polyn. order	5	3	2	2	1	1	1
Kernel	Uniform	Uniform	Uniform	Triangular	Uniform	Triangular	Triangular
Eff. obs. left	6,474,411	1,621,819	939,282	470,060	610,303	470,060	221,256
Eff. obs. right	1,785,716	952,924	680,133	395,294	491,410	395,294	199,925

Notes: RD estimates for never-owner-occupiers at age 40 for years 2006–2015. Age 40 has been excluded in all specifications.

the reduction in the probability of first-time home purchase is estimated to be between 0.32 and 0.68 percentage points. The 95% confidence intervals from these specifications cover the range from 0.21 to 0.92 percentage points. Usually, the kernel choice should make little practical difference (Lee and Lemieux, 2010), but we note that changing the uniform kernel in column (5) to the triangular one in column (6) changes the estimate noticeably. In this case, the change seems to be driven by the discrete nature of the running variable and the slight curvature just below age 40.

Although, the estimates reported in Table 3 may at first seem rather small, so is the probability of becoming a first-time homebuyer around age 40 among never-owner-occupiers. At ages 39 and 41, the share of first-time homebuyers is only 2% and 1.3%, so the estimates are substantial in relative terms. All the specifications in Table 3 yield the same qualitative conclusion: under continuity of CEFs assumption, the transfer tax exemption leads to a sizable increase in the probability of first-time home purchase at the age threshold of 40 years. Appendix Figure A.1 shows that above age 40 the CEF is similarly estimated in all specifications, and the differences in the estimates are driven by different approximations to the CEF below age 40. Two of the most conservative specifications in Table 3 in columns (5) and (6) estimate that the probability of becoming a first-time homebuyer at age 40 without tax exemption is roughly 1.36%. The estimates from the corresponding specifications suggest that tax exemption increases the probability of first-time homeownership among never-owner-occupiers at age 40 by 23.5%–30.9%. This should, however, be interpreted carefully. Below we discuss how the policy may affect timing decisions of when to become a first-time homebuyer. If such timing decisions are prevalent, the estimates may not reflect long-term changes in the homeownership rate.

Appendix Table A.5 presents the corresponding results for the years 2011–2015, for which linking the transaction data with the background covariates in FOLK succeeded without irregularities. These estimates are largely the same as those presented in Table 3. Noticeably, the corresponding estimates for specifications in columns (5) and (6) are .0033 and .0042 for the years 2011–2015. These are almost identical with the estimates .0032 and .0042 in Table 3 for years 2006–2015. This relieves the concern that RD estimates presented in Table 3 were somehow driven by the failures in linking the two data sets for the years 2006–2010. If we repeated the estimation but restricted the sample to those individuals who had been living as tenants for the previous 5 years instead of never-owner-occupiers, we would also receive a similar estimate, .0043, with 95% CI of [.0029, .0056], corresponding to the specification (6) in Table 3. Although these groups may largely consist of the same individuals around age 40, they are not entirely the

same: the tenant group is likely to contain more misclassifications when it comes to the eligibility status.

5.2 Sensitivity and falsification tests

Next, we conduct sensitivity analysis by validity/falsification tests that are common in studies that use RD design (Cattaneo et al., 2019; Imbens and Lemieux, 2008; Lee and Lemieux, 2010; van der Klaauw, 2008). The idea is that although we cannot directly test whether the average potential outcomes are continuous at the threshold or not, we use what we can observe to assess the credibility of the continuity assumption. However, the discreteness of the running variable causes some difficulties with bandwidth selection, as discussed below.

5.2.1 Continuity of predetermined covariates

It is well known to apartment buyers that 40-year-olds and older are ineligible to claim the transfer tax exemption. The policy is transparent and has been in effect since 1991. It is certainly plausible that individuals who in absence of the policy would have otherwise become first-time homebuyers after age 40 shift their purchase decision ahead in time to benefit from the tax exemption. If such “manipulation of age at purchase” takes place, it may invalidate the assumption of continuous average potential outcomes. Therefore, it is common in RD studies to assess whether there are discontinuities in predetermined covariates that cannot have been affected by the treatment, here, by tax exemption eligibility. If we do not find discontinuities in predetermined covariates this in itself does not imply that the CEFs are continuous. However, were we to find discontinuities that we cannot substantively explain, it would call the credibility of the continuity assumptions into question (Imbens and Lemieux, 2008).

Table 4 presents RD estimates and 95% confidence intervals for selected covariates using the same local linear regression with triangular kernel and bandwidth 5 as in column (6) in Table 3. These covariates are selected from the many presented in Table 1 because they might correlate with the decision to become a first-time homebuyer.

For these covariates, all the 95% confidence intervals, except one, contain 0. This leads us to conclude that there is not enough evidence to reject the hypothesis that these covariates are not discontinuous at age 40. The only covariate that is statistically significant from zero is disposable income. On one hand, if the null hypothesis of no discontinuities were true, and we conducted 15 independent significance tests, there would be roughly a 54% probability that at least one of them differs statistically sig-

Table 4: Potential discontinuities in predetermined covariates.

Covariate	Estimate	95% Conf. Int.
Urban municipality	-.0007	[-.0053, .0040]
Inner city	.0003	[-.0052, .0057]
Household size	0.0022	[-.0156, .0200]
Number of rooms	0.0000	[-.0145, .0145]
Female	.0003	[-.0051, .0057]
Disposable income	162	[42, 282]
Not part of a family	.0008	[-.0043, .0060]
Employed	.0011	[-.0044, .0065]
Manual workers	.0004	[-.0046, .0054]
Lower-level employees	.0006	[-.0043, .0054]
Multi-storey building	.0005	[-.0048, .0060]
Detached house	-.0017	[-.0058, .0023]
Upper secondary level	-.0010	[-.0064, .0044]
Tertiary education	.0018	[-.0020, .0055]
High standard of equipment	-.0001	[-.0040, .0038]

Notes: Predetermined covariates for year t get their values from the end of year $t - 1$. All the specifications are estimated by local linear regression that uses triangular kernel and bandwidth 5. The effective number of observations on the left of the threshold is 470,060 and on the right 395,294.

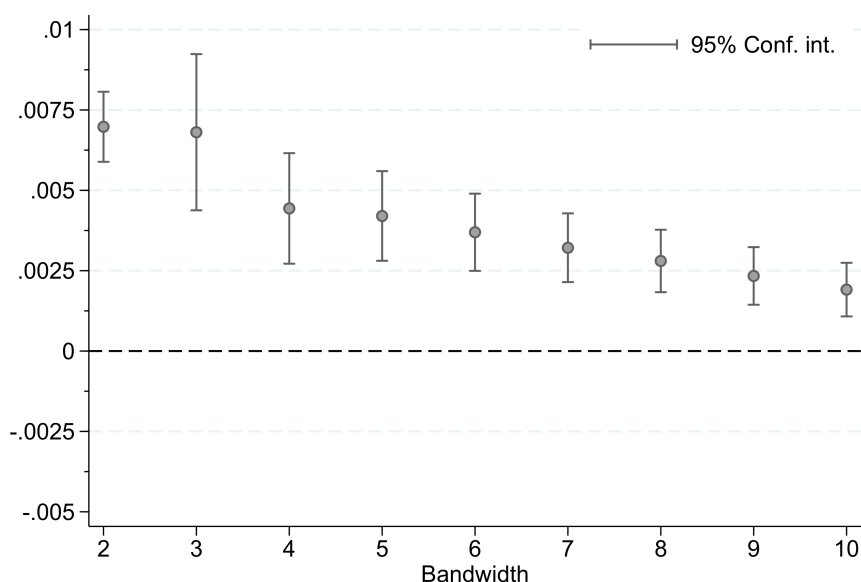
nificantly from zero. On the other hand, one might expect that disposable income is highly correlated with the decision to become a first-time homebuyer, and if discontinuities were known to exist, disposable income would be among the first variables to look at. It is possible that using the specification (6) in Table 3 does a poor job when it comes to approximating the CEFs of the predetermined covariates at age 40. However, based on Appendix Figure A.2 disposable income seems to be well approximated by the linear specification in the given bandwidth because there is hardly any curvature close to the threshold. It would be of interest to include disposable income and other predetermined covariates into the specification and see whether the point estimates and confidence intervals change. If the RD assumptions are valid, this should not affect the point estimate but improve the efficiency of the estimator (Calonico et al., 2019). To conclude, there is no clear evidence of discontinuities in predetermined covariates, but there is some ambiguity with disposable income. Of course, there may be characteristics that are discontinuous at the threshold, but we are just unable to observe them.

5.2.2 Sensitivity to bandwidth choice

Next, we study how sensitive the results are to bandwidth selection, which has by now become standard in RD literature (Cattaneo et al., 2019; Imbens and Lemieux, 2008;

Lee and Lemieux, 2010)). No matter how the initial bandwidth is chosen, results that are not heavily dependent on a particular bandwidth are more credible than results that are sensitive to bandwidth choice (Imbens and Lemieux, 2008). Table 3 presented specifications with different polynomials and bandwidths but here we specifically study the sensitivity of the linear specification with triangular kernel. Increasing the bandwidth causes observations farther away from the threshold to affect the estimate. This typically increases the bias of the linear approximation at the boundary, but it also leads to smaller standard errors due to increased sample size. This is sometimes referred to as a bias-variance trade-off (Cattaneo et al., 2019). In this setting, increasing the bandwidth too much with linear specification leads to bad boundary approximations below the threshold because of the curvature of the CEF. Figure 3 presents graphically the estimates and 95% confidence intervals for bandwidths between 2 and 10, and Appendix Table A.3 presents the numerical counterparts.

Figure 3: Sensitivity to bandwidth choice.



Notes: Estimated with local linear regression using triangular kernel. Bandwidths 3 and 5 correspond to the specifications (6) and (7) in Table 3.

The point estimates in Figure 3 vary between .0019 and .0070 as the bandwidth changes. Notably, all the point estimates and their 95% confidence intervals remain positive even for the largest bandwidth of 10. Qualitatively the results are consistent with the bias-variance trade-off, increasing the bandwidth leads to narrower confidence intervals but the linear approximation yields increasingly biased estimates, and the con-

fidence intervals are not centered correctly. Estimates based on bandwidths 2 and 3 are quite large in magnitude, .0068 and .0070, but increasing the bandwidth to 4 leads to a noticeable reduction. Any consecutive increase of bandwidth decreases the point estimate, but nowhere as much as the increase from 3 to 4. Appendix Table A.3 presents the results also for local quadratic regression, and Appendix Figure A.3 presents the results graphically. All the point estimates are positive and above .0046. The estimates based on the quadratic specification are generally more stable and less sensitive to increasing bandwidth from 4 to 10 — the more flexible polynomial is better at accounting for the underlying nonlinearities.

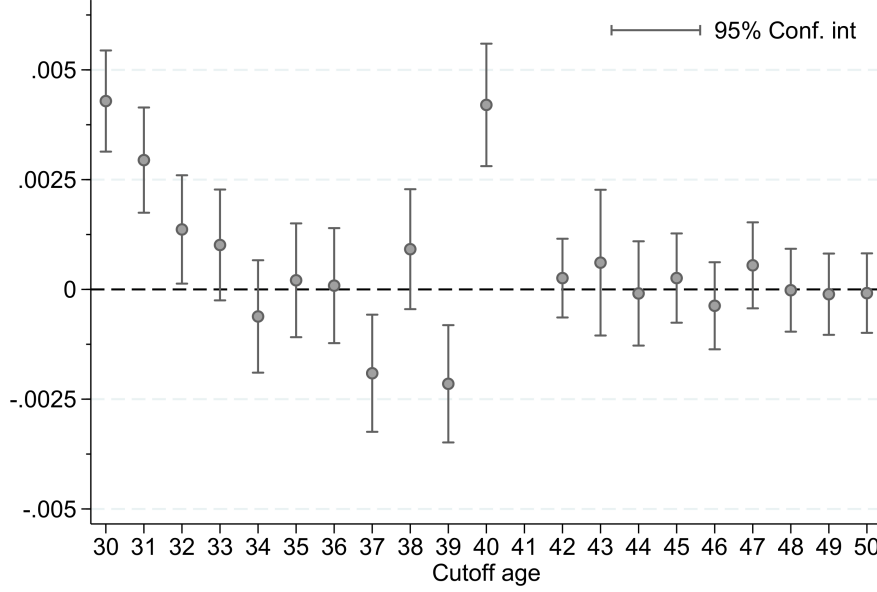
5.2.3 Sensitivity to alternative cutoff values

Another common approach is to study whether the CEFs are continuous at those values of the running variable, where we know for a fact that the treatment status does not change discontinuously. In other words, we study whether there are discontinuities in first-time homebuying decisions at ages other than 40. If the evidence suggests that the CEFs might be discontinuous at other values, it does not immediately disprove the validity of the continuity assumption at the threshold. However, if these discontinuities cannot be substantively explained it casts doubt on whether it is credible to assume continuity at the threshold (Cattaneo et al., 2019). Based on Figure 2 we might not suspect discontinuities at values other than 40, but we nevertheless conduct this typical sensitivity test.

For this test, we only use observations on either side of the threshold, except at age 40 which is graphed for easier comparison of the estimates. Therefore, the individuals that are used in these estimations either are all eligible or all ineligible to claim the tax exemption. Figure 4 presents the results graphically and Appendix Table A.4 contains the numerical counterparts. These estimates are again conducted with the same specification as the specification (6) in Table 3.

Based on Figure 4, there exists no evidence of discontinuities above age 40, but things are clearly different below. At face value, one might conclude that there seem to exist discontinuities at ages 30–32, 37, and 39. However, as seen in Figure 2, at the youngest ages, the observed discontinuities are driven by the linear specification and bandwidth choice. Linear approximations around the inherently nonlinear “peak” area just below age 30 are bound to produce RD estimates that are not credible. Using either quadratic specification or narrower bandwidth than 5 is enough to make the RD estimates for ages

Figure 4: Sensitivity to alternative cutoffs.



Notes: All placebo ages are estimated using local linear regression with triangular kernel and bandwidth 5. Estimates for ages 30–39 (41–50) are computed using only observations below (above) age 40. There is no estimate for age 41, because the data contains no observations with age below 41 but above 40.

30–32 close to zero and statistically insignificant.¹⁸ For ages 37 and 39, Figure 4 suggests that there are statistically significantly more first-time homebuyers at these ages than what would be expected from a continuous CEF that is well approximated by local linear regression. On one hand, the significantly positive RD estimate at age 39 could be an artefact of the same nonlinearity issue. On the other hand, it is also consistent with the hypothesis that some individuals who otherwise would have bought their first homes after age 40 shift their purchase decisions ahead in time to benefit from the tax exemption. It is unclear why there would be a discontinuity at age 37. Ideally, the age variable would be at a less aggregated level, so that it would be possible to credibly utilize data-driven optimal bandwidth estimators to avoid ambiguous results that could be driven by applying the same ad-hoc bandwidth at all cutoff values. Finally, it is worth noting that the RD estimate at age 40 (.0042) is much higher in absolute terms than the troublesome estimates at ages 37 (−.0019) and 39 (−.0021).

¹⁸Unfortunately, I did not manage to get the graphs and tables in time to include them in the Appendix.

5.2.4 Sensitivity to observations near the cutoff

Some individuals who without the tax exemption policy would have bought their first home only after age 40 may have conducted their home purchase below age 40 in order to benefit from the tax exemption. This is a real possibility because the age at which individuals are able to make their first-time home purchase decision is not randomized. Therefore, some individuals are able to self-select themselves into tax exemption eligibility by simply waiting for a shorter amount of time before buying a home than what they would have waited if the policy had not existed. However, everyone who is planning to shift their home purchase to just below age 40 will not succeed; households face financial shocks and there is uncertainty in the quality and location of the available housing stock. If this sort of shifting takes place, the RD estimate no longer identifies the effect of the policy on the homeownership rate at age 40 relative to the counterfactual world without the policy (van der Klaauw, 2008). However, it still provides evidence that the policy has real consequences by affecting the timing of the first apartment purchase.

If this — typically referred to as manipulation of the running variable — takes place, it seems credible to assume that most of these shifted decisions happen just around age 40. If it is costly to move the purchase decision ahead in time and these costs increase in the amount of shifting, we would expect to see that many of the shifted purchases take place just below the threshold at age 39. One approach to studying the sensitivity of the results to this kind of manipulation is known as the “donut hole”. The idea is that the estimation is conducted by excluding the observations that are closest to the threshold and potentially affected by the manipulation. This goes somewhat against the intuition of the RD design. Typically, the observations closest to the threshold are considered to be the most informative of the CEFs at the boundary. Therefore, they are also given the most weight apart from the uniform kernel. At the same time, all the specifications in Table 3 are already estimated without 40-year-olds, which effectively excludes all those who purchased their first apartment in the same year that they turned 40 years old. So, to some extent, we already have excluded observations that are closest to the threshold, albeit this was motivated by a different concern.

Table 5 presents the RD estimates for donut hole radii between 0 and 3. It is easy to choose the different donut hole radii to experiment with when the age is measured in years, but this is also a restriction. Ideally, we would want to study the sensitivity at less disaggregated levels, e.g. excluding observations that are within 6 or 18 months away from age 40. The donut hole radius 0 in Table 5 correspond to specification (2) in Table 3. When the radius is extended to 1, the estimate decreases by more than half to .0026

Table 5: RD estimates using the donut hole approach.

Donut hole radius	Estimate	95% CI	Eff. sample	# Excluded	
				Left	Right
0	.0057	[.0035, .0078]	2,574,743	0	103,950
1	.0026	[-.0020, .0072]	2,365,839	108,065	204,789
2	.0120	[-.0019, -.0221]	2,153,562	221,256	303,875
3	-.0041	[-.0262, .0179]	1,935,374	341,229	402,090

Notes: These estimates are based on local cubic regression with uniform kernel and bandwidth 10. The last two columns show the number of observations that were excluded from the estimation due to the donut hole radius. See Appendix Figure A.4 for graphical presentations.

and becomes statistically insignificant. The specification with radius 1 excludes all the observations where the first apartment was bought in the same year the buyer turned 39, 40, or 41 years old. For illustration purposes, specifications with donut hole radii 2 and 3 are also included in Table 5. However, they produce very poor boundary approximations that have practically zero information value. This can be seen in Appendix Figure A.4, which graphs all the four donut hole specifications. To conclude, the estimate decreases from .0057 to .0026 when the donut hole radius is extended from 0 to 1, and it becomes statistically insignificant. However, the small number of mass points in the support of the running variable restricts the ability to properly study the sensitivity to different donut hole radii.

5.3 Discontinuities in other outcome variables

First-time homebuyers eligible for the transfer tax exemption may purchase apartments that differ in their characteristics from the apartments that they would have purchased had they not been eligible to claim the tax exemption. Perhaps, eligibility for the tax exemption pushed some individuals to purchase more expensive housing that was concentrated in inner cities, but the dwellings were smaller in size? Table 6 presents the RD estimates for eight variables that could have been affected by the treatment. The corresponding RD graphs are presented in Appendix Figure A.5. The first four variables from “inner city” to “multi-storey building” are from the FOLK data and are measured at the end of the apartment purchase year. Because to claim the tax exemption it is necessary to move into the dwelling within six months, likely, everyone who purchased their first home in a given year had not moved in by the end of the year. Especially those who purchased their home later in the year. Therefore, for some, the values in FOLK may not have been affected when these measurements were taken. Variables

Table 6: Discontinuities in alternative outcome variables.

Outcome variable	Estimate	95% CI	Eff. obs. left	Eff. obs right
Inner city	.0015	[-.0040, .0070]	470,060	395,294
Number of rooms	.0026	[-.0119, .0171]	438,868	367,651
Detached house	.0001	[-.0041, .0043]	470,060	395,294
Multi-storey building	.0007	[-.0046, .0061]	470,060	395,294
Area of purchased apts.	-.5740	[-2.651, 1.503]	11,387	5,561
Price per square meter	115.2	[34.14, 196.3]	11,383	5,557
Weighted price of purchased apts.	4515	[-765.1, 9796]	11,387	5,561
Value of owned apts.	3173	[-2967, 9314]	11,331	5,534

Notes: Few observations did conduct more than one apartment purchase within a year, and for those the average values are used. Weighted price refers to the purchase price weighted by the ownership share. Value of owned apts. is calculated as the value at the end of the year, which explains why the number of effective observations differs from the number of effective observations in Weighted price of purchased apartments. All the estimates are based on local linear regression with triangular kernel and bandwidth of 5.

“area of purchased apartments” to “value of owned apartments” come from the Tax Administration’s data and are therefore only calculated for those never-owner-occupiers who bought apartments within the calendar year.

Based on Table 6, the hypotheses that transfer tax exemption does not affect the number of rooms or whether one lives in the inner city, in a detached, or multi-storey building cannot be rejected. The estimates suggest that never-owner-occupiers eligible for the tax exemption buy smaller, more valuable apartments, but only the price per square meter is statistically significant. The 95% confidence interval on price per square meter suggests that the tax exemption leads first-time homebuyers among never-owner-occupiers to pay €34.14 to €196.3 more per square meter. For comparison, this corresponds to an increase of 0.87%–5.00% relative to the mean price per square meter of old housing company dwellings in the capital region in the last quarter of 2020 (Statistics Finland, 2021a).

Individuals eligible for the transfer tax exemption effectively face lower housing prices than non-eligible individuals. This allows eligible buyers to outbid otherwise similar non-eligible individuals. Therefore, the finding that eligibility for the tax exemption causes individuals to pay a higher price per square meter is consistent with what might be expected. On the other hand, in the analysis of the predetermined covariates, there was ambiguity about whether those below the threshold may have had higher incomes, which could also imply higher wealth. This could also explain the difference in price per square meter. Additionally, all specifications were estimated using the corresponding specification to column (6) in Table 3, without further assessing how robust these results are. These results should therefore be interpreted carefully.

Table 7: Regression estimates for the logarithm of price.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Tax exemption	.0910 (.0022)	.0704 (.0023)	.0702 (.0022)	-.0193 (.0016)	-.0203 (.0016)	.0103 (.0018)	.0098 (.0020)
ln(area)		-1.350 (.1387)	-1.344 (.1386)	.3666 (.0960)	.3621 (.0947)	.5221 (.1013)	.3485 (.1090)
ln(area) ²		.2779 (.0171)	.2770 (.0171)	.0592 (.0119)	.0597 (.0117)	.0396 (.0125)	.0621 (.0135)
Year FE			x	x	x	x	x
Municipality FE				x	x	x	x
Year*municipality					x	x	x
Income & education						x	x
Addit. covariates							x
R^2	.0027	.2693	.2696	.6154	.6223	.6323	.6374
Observations	405,590	405,357	405,357	405,357	405,357	352,999	352,999

Notes: Data is from the years 2012–2015. Additional background covariates in specification (7) contain dummies for marital status, main type of activity (incl. employed, unemployed, student), family status and the number of under 18 years old children in the family. The constant terms are omitted. A detailed description of these variables can be found in https://taika.stat.fi/en/aineistokuvaus.html#!?dataid=FOLK_19872019_jua_perus20_002.xml.

5.4 Regression analysis of housing prices

In addition to the RD analysis around the eligibility age threshold of 40 years, I use multivariate regression analysis to assess how the housing prices paid in tax-exempted transactions differ from transactions without the tax exemption. The data is the same Tax Administration data on housing company shareholdings, but this time the sample covers transactions by all apartment buyers from years 2012 to 2015 instead of just by never-owner-occupiers. Years before 2012 are excluded because data on the municipality is only available starting from the year 2012, and 2016 is excluded for the same reason as before: failure in linking transaction data with buyer covariates. The dependent variable is the natural logarithm of the free-of-debt price, which allows small coefficients to be interpreted as approximately the percentage changes in apartment prices that are associated with being exempted from the 2% transfer tax. The results are reported in Table 7.

Specification (1) in Table 7 shows how tax-exempted apartment purchases are on average roughly 9% more expensive than non-tax-exempted purchases. Once we adjust for dwelling size and year-fixed effects in specifications (2) and (3), the coefficient on tax exemption drops slightly to 7%. Specifications (4) and (5) introduce municipality-

fixed effects and interactions between years and municipalities to adjust for different time trends in different municipalities. These introductions change the sign on tax exemption. This suggests that the earlier positive coefficients were driven by the fact that tax-exempted apartment purchases are largely conducted in municipalities with higher apartment prices. With the regression adjustments in specifications (4) and (5), tax-exempted apartment purchases were on average roughly 2% less expensive than transactions that were not tax-exempted.

Finally, specifications (6) and (7) in Table 7 introduce predetermined covariates of the apartment buyer. Specification (6) adds only disposable income and highest completed education, both measured at the beginning of the year in which the apartment purchase was conducted. Now, the coefficient on tax exemption is once again positive, suggesting that conditional on the other adjustments, tax-exempted transactions are on average 1% more expensive than transactions without the tax exemption. Specification (7) includes more predetermined covariates of the buyer: marital status, the main type of activity, family status, and the number of children below age 18. Although this changes the coefficient on the apartment size, the coefficient on tax exemption remains stable at 1%. The coefficient on tax exemption is statistically significant at the standard 5% significance level in every specification.

Can we conclude based on the estimates in specifications (6) and (7) in Table 7 that transfer tax exemption has a causal effect to increase purchase prices relative to what would have been paid in the presence of the transfer tax? There are at least a few caveats.

First, there may be unobserved heterogeneity between tax-exempted and non-tax-exempted apartment purchases. The tax exemption indicator may correlate with omitted variables that also affect the buyers' willingness to pay. Tax-exempted buyers have likely been subject to other policies that subsidize first-time homebuying. In the RD setting above, this was less of a worry, because only the eligibility for the tax exemption switches discontinuously off at age 40. For example, the ASP loan is a government-backed mortgage loan targeted to first-time homebuyers requiring buyers to save 10% of the dwelling price to specific ASP savings accounts. ASP loan has a government interest rate guarantee for 10 years. If the interest rate on the mortgage exceeds 3.8%, the government pays 70% of the exceeding part. Additionally, a certain percentage of the mortgage interest is tax-deductible. The deduction is foremost made from capital income. If there is no capital income, one receives a tax credit of 30% for earned-income taxes, but first-time homebuyers receive an additional 2 percentage points tax credit. If easier access to mortgage loans and lower associated risks affect the willingness of first-

time homebuyers to pay for apartments, then likely, this effect is also partly captured by the tax exemption indicator. This would lead us to overestimate the effect that transfer tax exemption alone has on prices.

Another worry is that we are adjusting for apartment size, motivated by the desire to compare how prices paid by tax-exempted and non-tax-exempted persons vary over similar apartments. Apartment size is, however, part of the same purchase decision with the price of the apartment. Therefore, it may be affected by the tax exemption itself. In this case, even if the tax exemption indicator does not correlate with subsidy policies, the estimate may be biased (Wooldridge, 2005). A similar argument can be made for adjusting for the municipality, which could also be affected by tax exemption.

6 Conclusions

The estimate for the preferred specification (6) in Table 3 suggests that the probability of first-time home purchase drops by 0.42 percentage points at age 40 among never-owner-occupiers. Because in this group the probability of first-time home purchase around age 40 is rather low, to begin with, this translates to a drop of roughly 30%. This may seem large at first, but as we learned in section 3.2, it is consistent with the estimates from previous studies, albeit it is from the higher end. Because the tax exemption corresponds to 2% of the free-of-debt price, this result resembles the finding from the UK that a 2 percentage point increase in stamp duty reduced household mobility by 37% (Hilber and Lyytikäinen, 2017). Due to the discreteness and aggregated nature of the age variable, it was not possible to credibly use data-driven optimal bandwidths, which is why ad-hoc bandwidths were used instead. Fortunately, the results from local linear and quadratic regressions were not generally sensitive to the bandwidth choice.

These results apply to a specific population of individuals who had not lived independently of their parents in owner-occupied dwellings after age 20. The identification of this subgroup may not have been perfect due to the issues discussed in section 4.2. Therefore, the eligibility for the tax exemption may not drop precisely drop from 1 to 0 at the threshold age of 40 as it should in sharp RD design, and may create downward bias. Furthermore, the RD design identifies the treatment effect at a specific point of the age distribution, at age 40, and it is not generalizable to other ages without further assumptions. It is also important to keep in mind that the data consisted only of housing company dwellings for which a transfer tax rate of 2% is applied, contrary to directly owned houses for which the rate is 4%. Directly owned single-family detached houses tend to be located in less densely populated areas than housing company dwellings, and the differences in populations and spatial characteristics provide a reason to worry about the external validity, even if the transfer tax rates were the same. Studying the effects of transfer taxes on directly owned single-family detached houses using high-quality Finnish microdata provides an interesting and fruitful avenue for future research. Also, it would be useful to explore whether and how the RD results would be affected if one was to introduce additional covariates into the specification.

The RD estimates generally used data from the years 2006 to 2015. Prior to March 1, 2013, the transfer tax rate for housing company dwellings was 1.6% and the tax base was the sales price, whereas afterward the tax rate was increased to 2% and the tax base was changed to the free-of-debt price of the dwelling. Therefore, the estimates based on the pooled data can be interpreted as the weighted average of these two different tax

rates and tax bases. Interestingly though, the point estimate of the linear specification (6) in Table 3 turned out to be the same as when the years were restricted to 2011–2015 (see Appendix Table A.5). Likewise, the confidence intervals are rather close to each other.

If the CEFs of the potential outcomes are continuous at the threshold, then the RD estimand identifies the causal effect of the homeownership rate at age 40 relative to a counterfactual world where the policy is not in place. However, because the policy is well understood by potential homebuyers, individuals who in absence of the policy would have purchased their first home after age 40 are incentivized to shift their purchase decision to below age 40 to claim the tax exemption. If this sort of behaviour is prevalent, the RD estimate cannot be interpreted relative to a counterfactual world without the policy in place. We found some nonconclusive evidence of this possibility.

In general, we could not reject the continuity of predetermined covariates at age 40 but results in Table 4 suggested that disposable income might have a slight discontinuity at the threshold. Likewise, although generally there were no discontinuities at placebo age cutoffs, Figure 4 and Appendix Table A.4 suggested that there were more first-time homebuyers at age 39 (and 37) than we would have expected from a continuous CEF. It seems credible that if transfer tax exemption pushed individuals to shift their first apartment purchase ahead in time, it is more likely to take place close to the 40-year eligibility threshold. When we excluded observations within a one-year radius from the threshold, the point estimate decreased and did not differ statistically significantly from zero.

We also explored alternative explanations for these anomalies based on multiple hypothesis testing and using a coarse age variable that caused difficulties in bandwidth selection. Besides, all specifications in Table 3 already excluded those observations where the buyer was 40 years old at the end of the purchase year. After also excluding buyers who were 39 and 41 years old at the end of the purchase year, the point estimate remained positive, and it was not much below the smallest point estimates in Table 3.

Finally, we briefly studied how the free-of-debt prices differed between tax-exempted and non-tax-exempted apartment purchases. The regression adjusted for apartment size, year and municipality-fixed effects, and their interactions, as well as covariates of the buyers. These covariate-adjusted specifications (6) and (7) in Table 7 suggested that tax-exempted housing company dwelling purchases were 1% more expensive than transactions without tax exemption. There are at least two caveats with causal interpretation. The first is the omitted variable bias that troubles the majority of the benefits of homeownership literature. First-time homebuyer’s tax exemption is likely to be cor-

related with other subsidies to first-time homebuyers. Second, we may have adjusted for variables that are affected by the tax eligibility itself. These two sources may bias the estimate, but the sign of the bias remains ambiguous.

The literature on the effects of transfer taxes in section 3.2 suggests that it is sellers who largely bear the burden of transfer taxes, and this may be especially true for apartments that are bought for investment purposes. However, when it comes to the effects on prices paid by first-time homebuyers, it is important to note that most housing transactions are conducted by buyers who are not exempted from transfer taxes. Only some buyers have improved purchasing power due to the tax exemption, and this explains why we may not see as large effects on the prices that first-time homebuyers pay as in studies where changes in tax rates apply to everyone. It would be fruitful to study whether similar findings would hold for directly-owned single-family detached houses that are taxed at a higher rate, and to further study this issue with a better identification strategy.

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Appendix

Table A.1: Frequencies and shares of failed merges by year.

	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016
Panel A.											
Freq.	28,995	25,089	17,339	11,651	6,795	2,558	471	1	1	0	46,116
%	20.85	18.04	12.47	8.38	4.89	1.84	0.34	0.00	0.00	0.00	33.2
Panel B.											
Freq.	11,558	10,040	6,666	4,051	1,954	497	60	0	1	0	20,196
%	33.54	29.24	23.50	14.52	7.19	1.67	0.22	0.00	0.00	0.00	95.72

Notes: Panel A shows the frequencies and shares of all housing company dwelling purchases that could not be merged successfully with FOLK data because of mismatches in encrypted personal identifiers. Panel B shows the corresponding frequencies and shares but only to those transactions that claimed the transfer tax exemption. Transactions in both panels were conducted by 18–39-year-olds.

Table A.2: Joint distribution of the proxy and tax exemption indicator in 2011–2015.

Age bins	Proxy=0 Exempted=0	Proxy=1 Exempted=0	Proxy=0 Exempted=1	Proxy=1 Exempted=1	# Apt. purchases
20–24:	8.38	5.03	0.18	86.41	28,809
25–29:	9.81	4.44	0.17	85.58	45,771
30–34:	9.17	5.36	0.20	85.27	22,597
35–39:	8.63	9.09	0.33	81.94	7,807
20–39:	9.19	5.14	0.19	85.47	104,966

Notes: Joint distribution among 20–39-year-old never-owner-occupiers who conducted apartment purchases.

Table A.3: Sensitivity to alternative cutoffs.

Bandwidth	Linear		Quadratic		Eff. obs. left	Eff. obs. right
	Estimate	95% CI	Estimate	95% CI		
2	.0070	[.0059, .0081]	–	–	108,065	100,839
3	.0068	[.0044, .0092]	–	–	221,256	199,925
4	.0044	[.0027, .0062]	.0114	[.0067, .0162]	341,229	298,140
5	.0042	[.0028, .0056]	.0063	[.0032, .0094]	470,060	395,294
6	.0037	[.0025, .0049]	.0059	[.0034, .0083]	610,303	491,410
7	.0032	[.0021, .0043]	.0055	[.0035, .0076]	765,121	586,406
8	.0028	[.0018, .0038]	.0051	[.0033, .0069]	939,282	680,133
9	.0023	[.0014, .0032]	.0050	[.0033, .0065]	1,137,562	772,069
10	.0019	[.0011, .0028]	.0046	[.0031, .0061]	1,363,681	862,944

Notes: All specifications are estimated using triangular kernel. Linear specifications with bandwidths of 3 and 5 correspond to specifications (6) and (7) in Table 3. Figure 3 and Appendix Figure A.3 are based on this table. Bandwidths 2 and 3 with triangular kernel effectively use only one and two mass points, which necessarily constrains the quadratic terms to zero.

Table A.4: Sensitivity to alternative cutoffs.

Cutoff	Estimate	95% CI	Eff. obs. left	Eff. obs. right
30	.0043	[.0031, .0054]	1,439,187	1,011,516
31	.0029	[.0017, .0041]	1,268,323	893,621
32	.0014	[.0001, .0026]	1,112,775	796,333
33	.0010	[-.0002, .0023]	976,169	718,026
34	-.0006	[-.0019, .0007]	856,698	657,056
35	.0002	[-.0011, .0015]	753,378	610,303
36	.0001	[-.0012, .0014]	667,502	470,060
37	-.0019	[-.0032, -.0006]	598,053	341,229
38	.0009	[-.0004, .0023]	543,865	221,256
39	-.0021	[-.0035, -.0008]	502,238	108,065
40	.0042	[.0028, .0056]	470,060	395,294
42	.0003	[-.0006, .0012]	100,839	485,567
43	.0006	[-.0011, .0023]	199,925	480,208
44	-.0001	[-.0013, .0011]	298,140	473,929
45	.0003	[-.0008, .0013]	395,294	467,650
46	-.0004	[-.0014, .0006]	390,571	461,514
47	.0005	[-.0004, .0015]	386,481	455,505
48	-.0000	[-.0010, .0009]	381,993	449,511
49	-.0001	[-.0010, .0008]	376,775	443,973
50	-.0001	[-.0010, .0008]	371,534	438,439

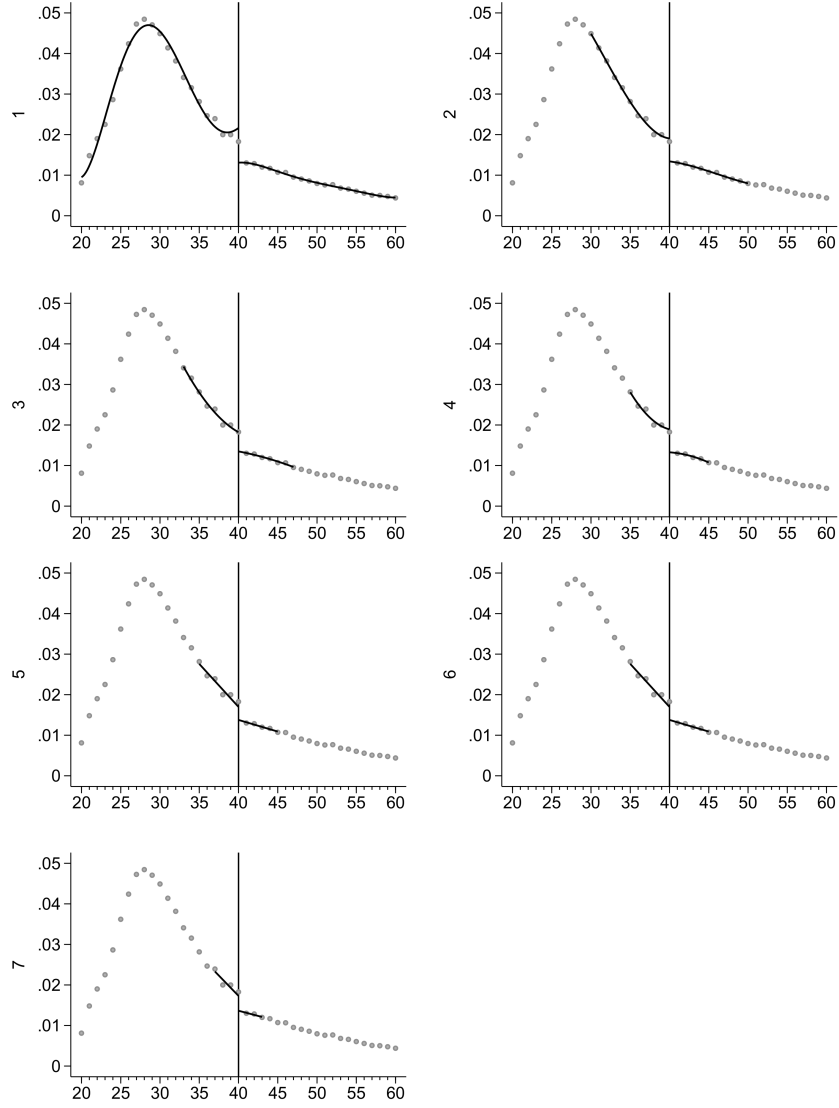
Notes: All the different cutoffs use the same specification of local linear regression with triangular kernel and bandwidth 5. Only age 40 is estimated using observations at both sides of the threshold. Cutoff ages 30–39 (42–50) use only observations below (above) age 40.

Table A.5: RD estimates for the years 2011–2015.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Estimate	.0076	.0054	.0048	.0031	.0033	.0042	.0042
95% CI	[.0044, .0108]	[.0025, .0083]	[.0024, .0071]	[-.0012, .0074]	[.0017, .0049]	[.0022, .0061]	[.0008, .0076]
p-value	.0000	.0003	.0000	.1520	.0000	.0000	.0145
Bandwidth	20	10	7	5	5	5	3
Polyn. order	5	3	2	2	1	1	1
Kernel	Uniform	Uniform	Uniform	Triangular	Uniform	Triangular	Triangular
Eff. obs. left	3,326,933	859,250	492,120	239,328	314,466	239,328	109,033
Eff. obs. right	855,018	455,513	322,995	186,061	231,905	168,061	93,834

Notes: RD estimates for never-owner-occupiers at age 40 for years 2011–2015. Age 40 has been excluded in all specifications. This corresponds to Table 3 that presents results using all years.

Figure A.1: Graphical presentations of the RD specifications.



Notes: Notes: This figure uses data from years 2006–2015. Age 40 has been excluded in all specifications. The bandwidths, kernels and polynomial orders vary corresponding to Table 3. Bandwidths are reflected in the support of the curves as they are only graphed for values within the bandwidth.

Figure A.2: Potential discontinuities in predetermined covariates.

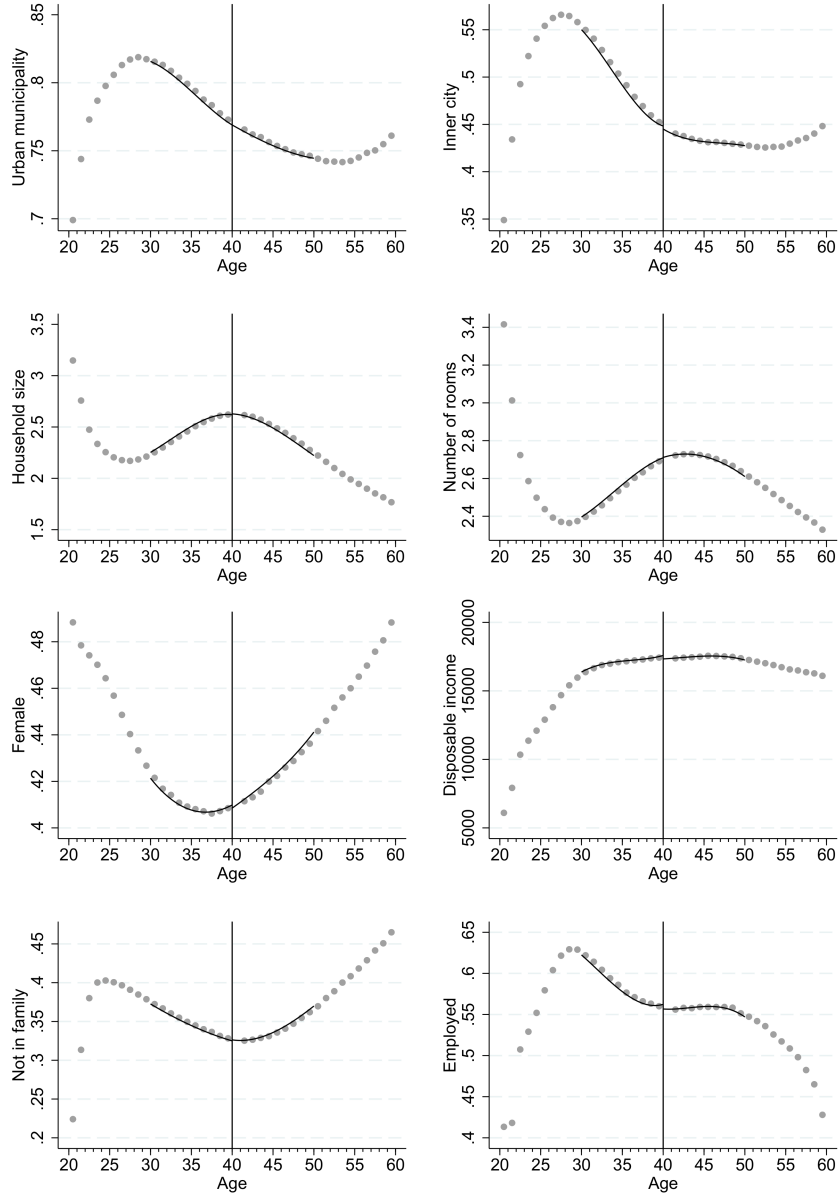
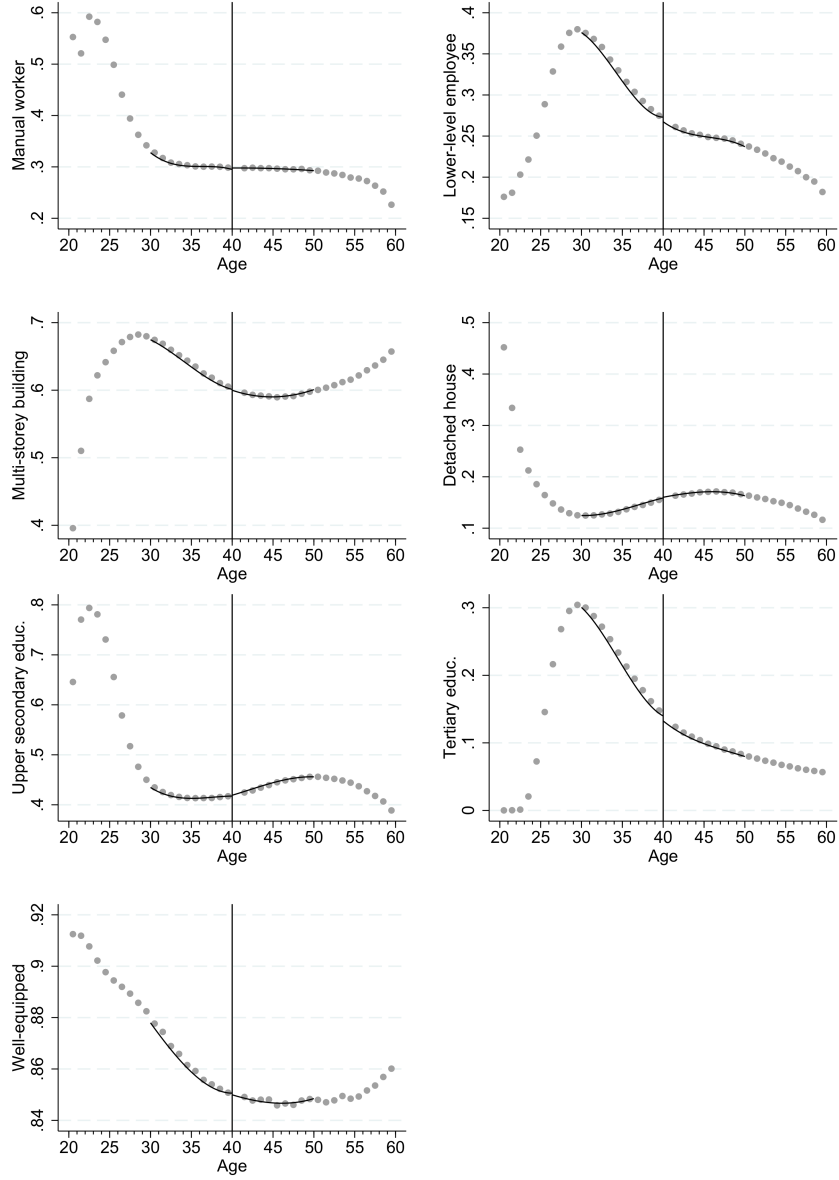
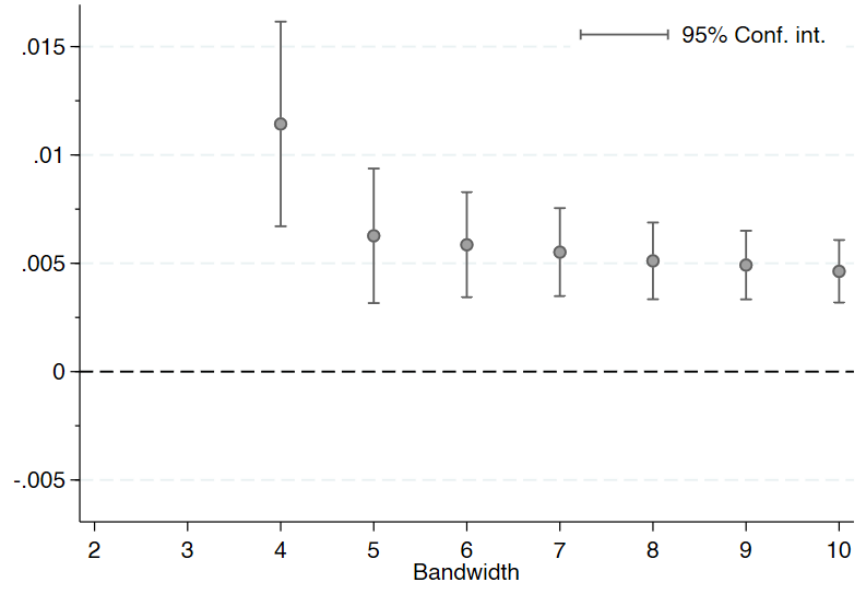


Figure A.2: Potential discontinuities in predetermined covariates (cont.).



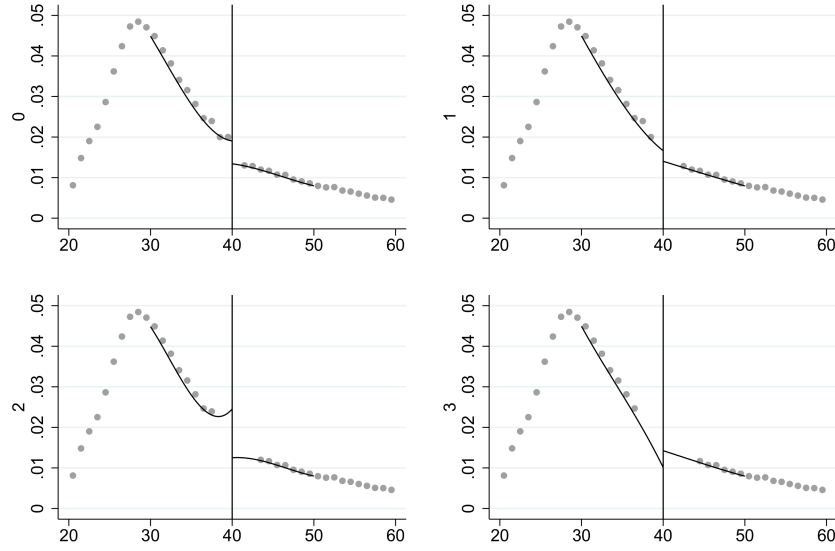
Notes: All the specifications are estimated by local linear regressions with triangular kernel and bandwidth 5. The variables correspond to those presented in Table 4. The values for these covariates were measured at the last day of the year before dwelling purchase.

Figure A.3: Sensitivity to bandwidth choice using local quadratic regression.



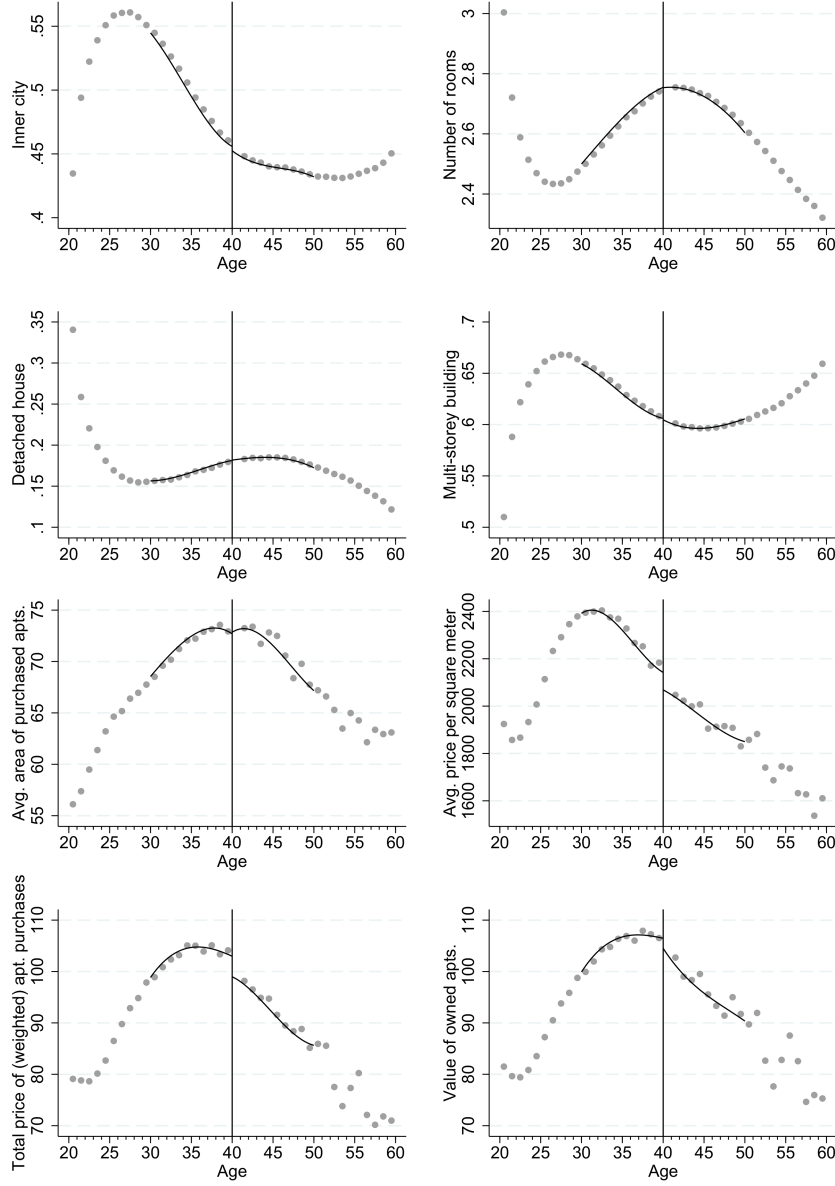
Notes: Estimated with local quadratic regression using triangular kernel. Estimate with bandwidth 5 corresponds to the specification (5) in Table 3.

Figure A.4: Graphical representation of the donut hole approach.



Notes: Graphical presentations for Table 5. The number on the y-axis corresponds to the donut hole radius. The specifications are estimated using third order polynomial, uniform kernel and fixed bandwidth of 10. It is clear that especially below the threshold the boundary approximations for radii 2 and 3 differ wildly and are not credible.

Figure A.5: Discontinuities in alternative outcomes.



Notes: The subgraphs correspond to the alternative outcome variables that are presented in Table 6. Total price of weighted apartment purchases and the value of owned apartments at the end of the year are measured in thousands of euros. All the variables are measured at the end of the year.